REDISCOVERING THE HISTORY OF PSYCHOLOGY

Essays Inspired by the Work of Kurt Danziger

Edited by

Adrian C. Brock

Johann Louw

and

Willem van Hoorn

Rediscovering the History of Psychology

Essays Inspired by the Work of Kurt Danziger

HISTORY AND PHILOSOPHY OF PSYCHOLOGY

Series Editor: **Man Cheung Chung**, *University of Plymouth, Plymouth, United Kingdom*

DESTINED FOR DISTINGUISHED OBLIVION

The Scientific Vision of William Charles Wells (1757–1817)
Nicholas J. Wade

REDISCOVERING THE HISTORY OF PSYCHOLOGY

Essays Inspired by the Work of Kurt Danziger
Edited by Adrian C. Brock, Johann Louw, and Willem van Hoorn

Rediscovering the History of Psychology

Essays Inspired by the Work of Kurt Danziger

Edited by

Adrian C. Brock
University College Dublin
Dublin, Ireland

Johann Louw
University of Cape Town
South Africa

Willem van Hoorn
University of Amsterdam
Amsterdam, Netherlands

Created in the United States of America

Visit Springer's eBookstore at: http://ebooks.springerlink.com
 and the Springer Global Website Online at: http://www.springeronline.com

CONTRIBUTORS

Betty M. Bayer is Associate Professor of Social Psychology in Women's Studies at Hobart and William Smith Colleges, Geneva, New York, USA.

Richard Walsh-Bowers is Professor of Psychology at Wilfred Laurier University, Waterloo, Ontario, Canada.

Adrian C. Brock is College Lecturer in Psychology at University College Dublin, Ireland.

Kurt Danziger is Professor Emeritus of Psychology at York University, Toronto, Canada and Honorary Professor of Psychology at the University of Cape Town, South Africa.

Willem van Hoorn is Professor Emeritus of Psychology at the University of Amsterdam, Netherlands and Honorary Professor of Psychology at the University of Cape Town, South Africa.

Johann Louw is Professor and Head of the Department of Psychology at the University of Cape Town, South Africa.

Hans van Rappard has taught history and systems of psychology at the Free University, Amsterdam, Netherlands. He now studies comparative philosophy.

Irmingard Staeuble is Professor of Psychology at the Free University, Berlin, Germany.

Henderikus J. Stam is Professor of Psychology at the University of Calgary, Canada.

Pieter J. van Strien is Professor Emeritus of Theory and History of Psychology at Groningen University, Netherlands and former President of the Archives of Dutch Psychology (ADNP).

Andrew S. Winston is Professor of Psychology at the University of Guelph, Ontario, Canada.

CONTENTS

INTRODUCTION[1]

Adrian C. Brock

BACKGROUND AND AIMS OF THIS BOOK

The three editors of this book are all related to Kurt Danziger in different ways. I was a PhD student with him at York University in Toronto from 1988 to 1993 and we have stayed in regular contact ever since. Johann Louw is based at the University of Cape Town where Danziger is a regular visitor. Willem van Hoorn, who was Louw's PhD supervisor at the University of Amsterdam, is also a regular visitor to the University of Cape Town. The three of us came together out of a mutual admiration for Danziger's work.

When we began to discuss the possibility of producing a book on Danziger's work, we were agreed that we did not want to produce a 'Festschrift' in the traditional sense of the term. There is, of course, no harm in producing such a book but we thought that there could be no greater tribute to Danziger than to make his work the focal point for a variety of contributions representing several areas of active research in history and theory of psychology. Although in recent years productive scholarship has flourished in this field (Richards, 2002a), this situation is not reflected in the readily available literature.

There are few broad discussions of the current state of history and theory of psychology, as well as its problems and future directions. For obvious reasons, scholars in this field tend to focus on the limited aspect of the history of psychology that forms the topic of their research. The essays in this volume will go some way towards filling this gap. They range in scope from the role of history and theory of psychology in the discipline of psychology, the marginalization of cultural-historical approaches to psychology, historical psychology and its relationship to history and theory of psychology, the epistemological implications of critical history, the inclusion of parts of the world other than Europe and North America

in psychology's history, the future of academic disciplines and much more. As such, the essays in this volume can serve as a departure point for those who wish to acquaint themselves with some of the most important issues in the field.

KURT DANZIGER'S WORK

Kurt Danziger has had a long career in psychology, having been awarded his DPhil from the University of Oxford in 1952. The research for his dissertation involved standard 1940s laboratory experiments using rats (e.g. Danziger, 1953). He had already become critical of this kind of research while he was writing his dissertation and he subsequently began to do Piagetian-style research with children (e.g. Danziger, 1958). However, his interests moved towards social psychology during the 1950s and this area of psychology became his main research interest until the end of the 1970s. His work in this area includes a well-known study of the sociology of knowledge in South Africa (Danziger, 1963) and books on *Socialization* (Danziger, 1971) and *Interpersonal Communication* (1976). While much of this work is highly original and of continuing interest to researchers in this field (see Louw, this volume), the focus of this book is the work on history and theory of psychology that Danziger started to publish in 1979.

Danziger's switch to history and theory of psychology began when he had a sabbatical in academic 1973–74. He decided to use the sabbatical to acquaint himself with the original works of some of the important figures in the early history of psychology. His knowledge of German was an obvious advantage in this task as he read the work of Helmholtz, Fechner, Wundt, and many less prominent authors. Danziger compared his situation on reading these works to that of a subject in an Asch conformity experiment since the views that were being expressed in the original works of these authors bore little or no relationship to the views that had been traditionally attributed to them. At first, he wondered if he was misunderstanding these works but it became increasingly clear that there was a discrepancy between the primary and secondary sources (Brock, 1995a; 1995b).

It is difficult for those of us who were not involved in history and theory of psychology in the 1970s to imagine how undeveloped the field was at this time. Although history and theory of psychology had been an active area of pedagogy for many years, it had only become a recognized area of research in the United States in the late 1960s with the establishment of the American Psychological Association's Division 24 (Theoretical/Philosophical Psychology) and Division 26 (History of Psychology), as well as the Cheiron Society (International Society for the History of the Behavioral and Social Sciences), the *Journal of the History of the Behavioral Sciences* and the graduate program in history and theory of psychology at the University of New Hampshire (Ash, 1983; Brock, 1998). It was to be several more years before it became well established in other countries.

Danziger's move into history and theory of psychology in the early 1970s was a part of this wider trend.

The establishment of history and theory of psychology as an active area of research had several important consequences for the field. As Danziger's account of his reaction on reading the original works of important figures in the history of psychology shows, many historical 'facts' that had been merely taken for granted up to that point were challenged. Revisionist accounts of several important figures and events in the history of psychology began to appear and criticism of the standard accounts of psychology's history in the authoritative works by Boring (1950) and Allport (1985) was a central feature of what came to be known as 'critical history of psychology' (Danziger, 1984). Moreover, it was clear that the standard accounts of psychology's history helped to reinforce mainstream psychology and so revisionist history became a way of attempting to change the discipline itself.

Both of these points can be seen in what may be Danziger's best-known early work in this field, "The positivist repudiation of Wundt" (Danziger, 1979a). The standard view in American accounts up to that point was that the former student of Wundt, E. B. Titchener was a loyal disciple who had represented Wundt's views in the United States. This view had its origins in the text of Boring (1950) but Titchener's devotion to Wundt had been exaggerated even further by later writers (Brock, 1993). Danziger drove a wedge between the two by pointing out that Titchener had been influenced by the positivist epistemology of Mach, something that he had in common with Wundt's renegade student, Oswald Külpe who subsequently founded the Würzburg School. Danziger's account was based on sound historical scholarship but it can also be seen that he was enlisting Wundt's anti-positivist views in support of his own.

These themes were expanded in another article in the *Journal of the History of the Behavioral Sciences* in the following year, "The history of introspection reconsidered" (Danziger, 1980a). The title is taken from a paper by Boring titled, "The history of introspection" (Boring, 1953) and clearly shows Danziger's critical and revisionist aims. Another early work that deserves mention is a book chapter, "The social origins of modern psychology" that was published as part of a collection on 'sociology of psychological knowledge' since it shows the continuity between Danziger's early work on the sociology of knowledge in South Africa and his later historical and theoretical work (Danziger, 1963; 1979b; see also Louw, this volume). This chapter shows Danziger's familiarity with the sociology of science and includes a critique of the application of role theory in this area. A sociological orientation can also be seen in some of his later historical and theoretical work so that at times he has had to defend himself against the mistaken charge of 'sociological reductionism' (Danziger, 1992a; 1993a; see also Stam, this volume).

The years 1979/80 were important for the establishment of history and theory of psychology as a recognized sub-speciality within the discipline. According

to the traditional account of Boring (1950), the discipline of psychology could trace its origins to the establishment of Wundt's laboratory at the University of Leipzig in 1879. Psychologists as a whole are not generally interested in the history of their field but anniversaries are an exception to the rule and there was no bigger anniversary than the establishment of psychology itself. The American Psychological Association declared 1979 to be psychology's centennial year and the International Congress of Psychology, which is held every four years, moved to Leipzig in 1980 in order to commemorate the event. In spite of the dubious historical accuracy of this account, the anniversary provided many opportunities to publish historical work on Wundt and to have it widely read. Danziger contributed three chapters to two special volumes on the legacy of Wundt and an article in a special issue of *Psychological Research* that was devoted to Wundt (Danziger, 1980b; 1980c; 1980d; 1980e). As a result, he came to be regarded as one of the foremost Wundt-scholars in the field.

Danziger's work on Wundt declined sharply in quantity during the 1980s. I am aware of only two works on the subject that he published during this decade and both appear to have been commissioned (Danziger, 1983; 1988). Following the 'centennial' period, there was a change in Danziger's research interests towards psychological methodology and its history (e.g. Danziger, 1985a; 1985b). However, this interest was already apparent during the earlier period (Danziger, 1980e). The topic of methodology is central to psychology's history because of the theoretical divisions that became apparent in the early years of the discipline. The 1920s are sometimes characterized as 'the age of schools' and the theoretical diversity that existed was outlined by authors such as Woodworth (1931) and Heidbreder (1933). This kind of theoretical diversity is common in the human or social sciences, such as sociology, linguistics and anthropology, but psychologists were looking towards physics and the other natural sciences as a model for the discipline and described this situation as a 'crisis' (Driesch, 1926; Bühler, 1965; Vygotsky, 1985). It did not help the position of psychology in society since psychologists could hardly address the public from a position of authority if they could not agree among themselves. When psychology finally achieved some degree of unity after the Second World War, it was not on theoretical but on methodological grounds. A strict set of methodological rules was established in the United States and subsequently exported to other parts of the world. It was these methods that came to define the field.

The topic of 'mind' had been established for centuries as an object of philosophy and it was part of the discourse of society at large. Even 'behavior' could not be seen as the exclusive preserve of psychology since it was appropriated by a range of disciplines describing themselves as 'the behavioral sciences'. Therefore, the special contribution of psychology came to be defined not in terms of its subject matter but in terms of its methods. Even though many of these methods were unique to psychology (Winston, this volume), they were legitimated by an

appeal to 'science' and alternative methods were considered inferior at best and unacceptable at worst.

Danziger began his career as a psychologist shortly after the Second World War when these methodological prescriptions, and the intolerance of any alternatives to them, were at their height. With the sole exception of his early work with laboratory rats, Danziger had never felt bound by these strictures. His work with children used what has been called the 'clinical' method of Piaget and his work in the sociology of knowledge in South Africa had used non-traditional methods as well. He had always been critical of the primacy of method in mainstream psychology and sometimes used the term, 'methodolatry' to describe this situation. A useful introduction to Danziger's views on methodology is an article in *Philosophy of the Social Sciences*, with the title, "The methodological imperative in psychology" (Danziger, 1985b).

However, when Danziger began to make psychological methodology the main focus of his historical research, his criticisms moved to a different level. This change crystallized around 1983 and eventually resulted in Danziger's best-known work, *Constructing the Subject: Historical Origins of Psychological Research* (Danziger, 1990). It is virtually impossible to summarize such a rich work in a few paragraphs but some of its most salient points will be briefly mentioned. For Danziger, the psychology experiment using human participants constitutes a social situation exemplifying various social regularities. He contrasts the situation in the early German experimental research of Wundt and others where the participant was described as a 'research participant' or 'observer', rather than a 'subject'. The role of the participant was at least as important as that of the experimenter, as may be seen from the fact that these roles were often interchangeable. In some cases, the role of the 'observer' was more important than that of the 'experimenter' and this situation is reflected in the fact that the former was sometimes a person of greater social status than the latter. An example of this occurs in the famous experiments of the Würzburg School in which the head of the institute, Külpe often acted as the 'observer' in the experiments of someone like Bühler, who was officially his assistant (Bühler, 1907; 1908). It could even happen that an experimental report was published not by the experimenter but by the 'observer'; something that would be unthinkable in standard modern research. The term for the research participant that eventually came to be adopted in standard experimental research, 'subject' is not to be found in any of this early experimental work. It had previously been used in medical work on hypnotism and reflects the unequal division of power that occurs in the hypnotic situation. Thus the adoption of this term by experimental psychologists reflects a change in the division of power between the researcher and the participant.

Throughout this work, Danziger shows that the way of doing experiments that subsequently became enshrined as the *only* valid way of doing an experiment is merely one of several possible alternatives. He also shows that psychology has

always used a variety of investigative practices, of which experiments are only one, and can trace its history not only to Wundt's laboratory but also to the clinical work of Charcot in France and the psychological testing that was done by Galton in England. The history of these investigative practices indicates a gradual narrowing of research possibilities over the years. Moreover, the methods that eventually came to be adopted were adopted mainly for extraneous reasons and not on strictly scientific grounds. Using the phrase, 'marketable methods', Danziger shows how psychologists adopted methods that would yield results that would be of interest to the social institutions that had an interest in prediction and control.

Danziger's work has its parallels in recent work in the interdisciplinary area of 'science studies' that encompasses history, philosophy, sociology, and even anthropology, of science. Much of this work implies a critique of the quasi-religious status that science has acquired in some quarters and examines it as a social product. While some historians, philosophers and sociologists in the field of science studies have a broader agenda, Danziger is more concerned with psychology itself. If the accepted methods of mainstream psychology lose their quasi-religious status, then they too can be open to debate and the possibility of alternatives can be discussed. This difference in emphasis seems to be acknowledged when Danziger says that his approach owes much to the field of science studies but suggests that he may "have produced a different kind of insider's history" (Danziger, 1990; p. vii).

Even before *Constructing the Subject* had appeared, there was a noticeable shift in Danziger's interests towards what he originally called 'the history of psychological concepts and categories' and later called 'the history of psychological objects'. Danziger first became interested in this topic when he was a visiting professor at Gadjah Mada University in Jogjakarta, Indonesia from 1957 to 1959. He went there as an employee of the Indonesian government with the specific mandate to introduce 'western' psychology to the curriculum. To his surprise, he found that he had an Indonesian colleague who was teaching a local form of psychology called, 'ilmu djiwa' that was based on Hindu philosophy. Danziger suggested that they conduct joint seminars in which the local and the 'western' views of psychology could be compared but the joint seminars never took place because they could not find a common set of 'objects' around which the seminars could be based. The local psychology had no equivalents for the basic objects of English-language psychology, such as 'motivation', 'intelligence', 'personality' etc., and there were no equivalents in English-language psychology for the objects that were central to the local psychology. This seemed to be clear evidence that psychological objects were social products (Danziger, 1997a; see also Brock, 1995a; 1995b).

Many other examples of this phenomenon could be given. A topic that has been explored in some detail is the Japanese emotion of 'amae'. This emotion is very important in Japanese culture and many popular Japanese songs are based on it. There is no equivalent in English, or indeed in any other European language, for this emotion and it seems to be a specifically Japanese way of feeling (Morsbach

and Tylor, 1976). It is, of course, possible for non-Japanese persons to gain some understanding of what the emotion is about but it would take several paragraphs to explain it rather than one word. The insight that these concepts, categories or objects—whatever term is preferred—are social products leads to the obvious conclusion that they have a history as well. Harré (1983) has pointed to the existence of the now obsolete emotion of 'accidae' which was important in medieval Europe and which was manifested by a neglect of one's religious duties. Neglecting one's religious duties is less important to modern Europeans and this may explain why the emotion is now obsolete. Danziger has focussed not on psychological objects that are now obsolete but on the historical origins of the some of the most common objects of research in American psychology. In doing so, he has continued the process that he began in *Constructing the Subject* of historicizing aspects of psychology that are usually regarded as fixed and eternal. If the methods of psychology are viewed as sacred and not as social products that have a history, then this is even more true of the basic objects of psychological research.

Perhaps the first point that needs to be addressed is what exactly a 'psychological object' is in Danziger's view. A key text in this regard is an article that Danziger published in *Annals of Theoretical Psychology* in 1993 under the title, "Psychological objects, practice, and history" (Danziger, 1993a). In this work, Danziger defines psychological objects in social terms: "They are simply the things that psychologists take to be their proper objects of investigation or professional practice" (p. 24). It therefore follows that psychological objects vary from place to place and in different historical periods. One psychological object that Danziger has not examined but which can serve as an example of this phenomenon is 'stress'. This is now regarded as a major social problem in most developed countries and it is the object of a great deal of psychological research. However, until the middle of the twentieth century, the word had a purely physical meaning and referred to a force being exerted on a physical object. It has retained this meaning in terms such as 'stress fracture'. It was only after this term was applied metaphorically to human psychology around the middle of the twentieth century that it came to be regarded as a suitable object of psychological research (e.g. Selye, 1978). Thus 'stress' became a psychological object after this period, whereas previously it was not.

Danziger's main work on the history of psychological objects is his book, *Naming the Mind: How Psychology Found Its Language* (Danziger, 1997a). In this work, Danziger outlines the historical origins of several common objects of research in American psychology: behavior, learning, emotion, motivation, attitude, intelligence and personality. These are the kind of topics that might form the headings of the chapters in an American introductory text. Perhaps the most surprising result of this research is how 'modern' many of these concepts are. They are not much older than psychology itself. Although Danziger has tended to focus on psychological objects that were ultimately successful, he acknowledges that there have been failures as well. An example might be the 'Bewußtseinslagen' of

the Würzburg School, which have been mistranslated as 'imageless thought' but are more appropriately characterized as 'states of consciousness'. Towards the end of the book, Danziger suggests that the psychological objects that are currently popular in American psychology will eventually fall out of favor and be replaced by others.

In spite of there being a literature on the history of scientific objects by historians of science (e.g. Canguilhem, 1955; Smith, 1991, Daston, 2000), there has been virtually no work on the history of psychological objects by psychologists. This may be the outcome of what Danziger calls 'naïve naturalism'. This is the view that the current objects of English-language psychology correspond to some natural division of reality and can thus be regarded as 'natural kinds'. In place of this view, Danziger has adopted the term, 'human kinds' from his colleague in Toronto, Ian Hacking (e.g. Hacking, 1995; see also Danziger, 1999). An important characteristic of human kinds is that they not only help us to understand and to explain human action. They influence the action as well. It is probably of no importance to a dolphin whether we characterize it as a 'mammal' or a 'fish' but it is of great importance to parents who physically punish their children whether we characterize their actions as 'discipline' or 'child abuse'. One of the features of a human kind is that its application can sometimes be controversial. The person to whom it is applied can passively accept it or vigorously reject it. What both Hacking and Danziger want to emphasize here is the oft-stated view that human beings are 'self-defining creatures'. It is because of this that the activities of psychologists differ from those of their counterparts in the natural sciences since they are not merely attempting to describe a human nature that exists independently of the descriptions that they use. Their descriptions help to shape the phenomenon under investigation and it is here that the relationship between psychological objects and social practices lies.

Danziger has sometimes been mistakenly characterized as a 'sociological reductionist' for holding these views (Stam, this volume). While he clearly wishes to demonstrate that knowledge has a sociological dimension, he makes no claim to knowing what the ultimate nature of psychological knowledge is. The issue is seen as an empirical question that has yet to be resolved:

> Our only hope of establishing the reach of psychological knowledge is not to take its universality for granted at the outset, but to treat each of its products as a historically embedded achievement. Only when we understand something of this historical embeddedness of specific psychological objects and practices are we in a position to formulate intelligent questions about their possible transcendence. (Danziger, 1993a; p. 45)

Elsewhere, Danziger (1993b) suggests that trying to decide on these issues in advance of carrying out any historical or cross-cultural research is like trying to judge the outcome of a court case before the evidence has been produced. He has

also written positively of the critical realism of Roy Bhaskar (Danziger, 1990; see also Bhaskar, 1978; 1979). According to Richards (2002b), this philosophy "attempts to recoup the implications of social constructionism by accepting that the objects of knowledge are objectively real, but conceding that the terms in which they are known or knowable are in some sense socially determined" (p. 334). Thus the sociology of knowledge and philosophical realism are not incompatible, as is often supposed. The history of psychological objects as an area of historical research is compatible with a wide range of philosophical views and Danziger's realist position is only one possibility among several. However, it does need to be emphasized that when Danziger asserts that psychological objects are intimately related to the social practices of a particular time and place, he is referring to real social practices that have real effects on real people and not to some figment of our imagination.

Danziger has continued this line of research with his most recent work on the history of memory. This was a topic that he initially considered for inclusion in *Naming the Mind* but he came to realise that it was so vast that it needed a separate treatment (Danziger, personal communication). This work marks an important departure from the psychological objects that were examined in *Naming the Mind* in one very important respect: the concept of 'memory' is not a recent creation but has existed in one form or another since at least the time of Plato. According to Danziger (2002), the appearance of this term is connected with the social practice of storing information in written form. Plato's teacher, Socrates wrote nothing and relied on oral communication. It is no mere coincidence, therefore, that Plato introduced the concept with the metaphor of a wax tablet since it has always been linked to storing information in one form or another (see also Draaisma, 2000). The persistence of the concept can be explained in terms of the persistence of this social practice.

Danziger also shows that there have been wildly different conceptions of the phenomenon over time and he has recently returned to the topic of Wundt in order to illustrate this point. It is well known that Wundt did not carry out any memory experiments in his Leipzig laboratory. The start of experimental research on this topic is usually traced to the work of Ebbinghaus in Berlin (Ebbinghaus, 1885). This situation is often explained in purely technical terms; that is, Wundt did not develop the appropriate experimental techniques. Underlying this assumption is the view of naïve naturalism that memory has always been 'out there' and was merely waiting for someone to investigate it. According to Danziger (2001a), Wundt did not regard the topic of 'memory' as being of fundamental importance since, in his view, it was not one mental activity but the secondary product of several. It was a category of folk psychology—or what Wundt sometimes called, 'vulgar psychology'—and he dismissed it as an 'empty name'; that is, a word that had no proper referent. Wundt was not the only person in nineteenth-century Germany who held these views and they would not have appeared strange to Wundt's contemporaries.

Danziger has only published a small amount on this subject so far but it is already clear that he does not regard an apparently 'transhistorical' psychological object, such as memory, as being unaffected by socio-historical circumstances.

THE CHAPTERS

Some readers may be surprised by the use of 'history' and 'theory' throughout this introduction since these are sometimes seen as separate activities. This is particularly true in the United States where the American Psychological Association has separate divisions for these activities, Divisions 26 (History of Psychology) and 24 (Theoretical/Philosophical Psychology) respectively. This situation stands in sharp contrast to the institutional arrangements in the British and Canadian professional organizations, which have sections devoted to "History and Philosophy of Psychology". Danziger is a philosophically minded historian of psychology who has been a frequent participant not only in the meetings of these two sections but also in the meetings of the International Society for Theoretical Psychology. He has also published his work in journals such as *Theory and Psychology*, *Annals of Theoretical Psychology* and even *Philosophy of the Social Sciences* (e.g. Danziger, 1985; 1993a; 1994). In an interview that I conducted with him in 1994, he expressed the view that history without theory cannot be good history and that theory without history cannot be good theory (Brock, 1995a; 1995b). He has recently returned to this topic in a book chapter titled, "Where history, theory and philosophy meet: The historiography of psychological objects" (Danziger, 2003). As may be evinced from the title of this chapter, Danziger's work exemplifies this unified approach to history, theory and philosophy. Although Danziger's philosophical interests are evident in his historical work, he prefers to use the term, 'theory'; partly in order to distinguish it from that branch of philosophy called, 'philosophy of mind' or 'philosophical psychology' (Danziger, personal communication).

While the authors in this volume may differ in their views on how 'history' and 'theory' might be related, they are all united in the view that these activities should not be treated as distinct. Two of the authors in this book were the editors of a special issue of *Annals of Theoretical Psychology* that was devoted to exploring this relationship and two others contributed articles to this special issue (van Rappard & van Strien, 1993; Danziger, 1993a; Staeuble, 1993).

Hank Stam is well known for his contributions to both history and theory of psychology and as the editor of the journal, *Theory and Psychology*. He is therefore well qualified to discuss the theoretical implications of Danziger's work. As mentioned in the previous section, one aspect of this work that has been the object of much discussion in theoretical circles is its epistemological implications. While Danziger rejects charges of 'sociological reductionism' and regards himself as a philosophical realist, some psychologists have the impression that 'reality' is being glossed over or left out of his account. In this chapter, Stam argues that such

charges are unwarranted and that Danziger's epistemological views can be better understood if 'history' is taken as the departure point for these views rather than 'psychology' as it usually understood.

The chapter by Johann Louw examines what is chronologically the earliest work. Danziger's South African research exemplifying a sociology of knowledge approach provides some interesting links with his later historical work. As Louw points out, Danziger has continued to be a sociologically oriented historian of psychology and not just in his account of the 'peripheral' aspects of psychology, such as the history of psychologists and institutions. His sociological analysis extends to psychological knowledge itself and, although his work is generally characterized as 'history and theory of psychology', it can be seen as an exercise in the sociology of knowledge as well. Danziger's work is sometimes identified with what has come to be known as "The social constructionist movement in modern psychology" (Gergen, 1985). However, his sociologically oriented history of psychology has its roots in the much older tradition of the sociology of knowledge. He has suggested that it is more appropriate to regard the area of 'social construction' as an interdisciplinary field and that placing the suffix 'ism' on the end of this term can only raise false expectations about the amount of agreement that exists between the researchers from a variety of disciplines who work in this area (Danziger, 1997b).

Like Danziger, Andrew Winston has been working on the history of psychological methodology since the 1980s. They are also familiar with each other's work and there may have been some mutual influence since their research has overlapped. One topic that both have examined is the introduction of the concept of 'variable' into psychology (e.g. Winston & Blais, 1996; Danziger & Dzinas, 1998). Far from being a timeless and universal feature of science, the term was adopted by American psychologists in the 1930s and subsequently exported to other parts of the world. Winston also shows quite clearly that the term is a part of the internal culture of psychology and is hardly used in physics and other natural sciences. This work on the 'variable' concept is a good example of how the history of psychological methodology and the history of psychological objects can overlap. Winston also provides an account of a psychological object that died a very quick death: the 'experimentee'. The term was proposed by Saul Rosenzweig in the 1930s but he decided to abandon it following pressure from E. G. Boring. What is particular interesting about this account is the importance that was placed on the homogeneity of the methodological terms that were used within the discipline.

Pieter van Strien develops a different aspect of *Constructing the Subject* in his chapter on the single-subject research design. In his own work, Danziger had focussed on the historical origins of mainstream American research methods. One of the main features of these methods is that they take a large sample of 'subjects' and then work with the statistical averages from these results. This is equally true of experimental research that is interested in general human performance and in personality research where individual differences are the main focus of interest. Danziger described this situation as "the triumph of the aggregate" and

shows how the early German experimenters based their theories on evidence drawn from one participant or a small number of participants (Danziger, 1990). One of the most famous examples is Ebbinghaus who was both the subject and the experimenter in his memory research (Ebbinghaus, 1885). Danziger acknowledges that the single-subject research design has continued in psychophysics and van Strien expands these remarks. He also points out that the design has continued in other areas of psychology as well. Perhaps the best-known example is B. F. Skinner who frequently used a single animal in his research. Van Strien also refers to computer modelling, which belongs to a historical period that is later than the period that Danziger discusses in *Constructing the Subject*. The chapter provides an interesting extension of Danziger's work to other investigative practices. Van Strien acknowledges that psychologists like Skinner who did single-subject research were out of step with the majority of American psychologists and it is the methods of this majority that were the focus of Danziger's research. He also suggests that the persistence of the single-subject design can be explained in sociological terms.

Richard Walsh-Bowers is a former student of Danziger whose work in recent years has centered on research ethics and the social aspects of the research situation. The latter is an important focus of *Constructing the Subject* where Danziger had drawn attention to the research relationship in the early German experiments, which was a relationship of equals and sometimes a relationship in which the research participant had greater social status than the experimenter. It was only later that research participants came to be described as 'subjects', who had to be naïve and who were deliberately kept naïve by using deception and other strategies. Walsh-Bowers' aim is to introduce a greater degree of equality and democracy in the research situation and his chapter provides a good example of how critical history is often written with the aim of changing the present.

Following the publication of *Constructing the subject*, Danziger addressed a broad set of themes related to the historiography of psychology. In 1992, he presented a paper at a meeting of Cheiron-Europe titled, "In praise of marginality" (Danziger, 1992b). The paper discussed several aspects of marginality but perhaps the most important was the problematic status of history and theory of psychology in relation to psychology. This theme is taken up by Betty Bayer in her discussion of the prospects of a cultural-historical approach to psychology. Her work also touches on a theme that Danziger (1994) addresses in his paper, "Does the history of psychology have a future?". In this paper, Danziger suggested that it is important for critical historians to maintain a presence within psychology, and within science in general, so that they will be in a better position to have their views heard. Historians of science work in different departments from practising scientists, go to different conferences and publish in different forums. This is not an ideal position to be in if one wishes to influence the course of science. Bayer offers a somewhat depressing picture of scholars being hounded out of their academic disciplines and being forced to do their work elsewhere. Those who identify with critical

history will surely have different experiences in this regard but Bayer does end with an optimistic assessment of the prospect of change. Her chapter points to the importance of interdisciplinary work and interdisciplinary alliances. For those of us who take a sociological and historical perspective on these matters, disciplines are not 'natural kinds' that correspond to some natural division in the world but the product of social conventions that vary historically and cross-culturally. Even the label, 'psychologist' is a 'human kind' that one can accept or reject; or accept with qualifications.

The inclusion of Hans van Rappard in this volume is an indication of the editors' intentions of producing a critical discussion of Danziger's work. Van Rappard is well known as a critic of Danziger's approach to the history of psychology (van Rappard, 1997; 1998). In this chapter, van Rappard discusses the work of Wundt, a topic that was central to Danziger's early work in history and theory of psychology, and seeks to highlight what he considers to be the differences between the general approach of Danziger and that of his own. Van Rappard has criticised the trend among historians of psychology towards 'critical' history and he correctly views Danziger as one of the most prominent representatives of this approach. According to van Rappard, the most appropriate kind of work that a psychologist-historian (as opposed to a professional historian) can do is to examine the great theorists of the past in order to assist current theorizing and he offers his account of Wundt as an example of this approach. What complicates the situation considerably is that Danziger himself has used a similar approach in his discussions of Wundt's Völkerpsychologie (Danziger, 1983) or Lewin's early research in Berlin, which he has described as "buried treasure" (Danziger; 1990; p. 178). There is nothing inconsistent about being critical of mainstream psychology while simultaneously looking for alternatives among approaches to the subject that were historically less successful. Indeed, it could be argued that one is a necessary complement to the other in North America where to use the works of the past as a guide to the present is already a highly unorthodox step. While reading van Rappard's critique of Danziger's views on the history of psychology, and also that of Dehue (1998), it should be remembered that these Dutch authors work in a different social context from that of Danziger since this may explain some of the differences in their views.

One example of these local differences is the different status of historical psychology in (continental) Europe and the English-speaking world (Brock, 1995a; 1995b). Historical psychology has been an important theme in Danziger's recent work, though he was aware of its significance at an early stage (see Louw, this volume). This subject has long existed on the margins of English-language psychology (e.g. Barbu, 1960) but it is taken much more seriously in Germany (e.g. Loewenstein, 1992; Sonntag & Jütteman, 1993) and in the Netherlands (e.g. Verhave & van Hoorn, 1984; Peeters, 1996). Willem van Hoorn is a former student of Jan Hendrik van den Berg, a psychiatrist who represents a distinctive phenomenological approach to historical psychology (van den Berg, 1961), and he

has been engaged with this field for many years. His chapter is a plea for the inclusion of historical psychology as an integral part of the historiography of scientific psychology through the phenomenological concept of the 'life world'.

Irmingard Staeuble has been a prominent figure in the recent work on historical psychology in Germany (e.g. Staeuble, 1991; 1993). She is also well known as a critic of the postcolonial relationship between the so-called 'first' and 'third' worlds in psychology and has argued for a greater openness to non-western conceptions of knowledge. In this respect, her interests overlap with Danziger's own. It was Danziger's encounter with an alien form of psychological knowledge in Indonesia that led to his interest in the history of psychological objects. He has also criticized the unfortunate tendency to identify the history of American psychology with the history of psychology as a whole and has advocated what he calls a 'polycentric' approach to the field (Danziger, 1991; 1996). In this chapter, Staeuble outlines the expansion of western psychology around the world after the Second World War and the attempts to make it more appropriate to the local context under the label, 'indigenization' (e.g. Moghaddam, 1987). She also discusses the prospects of the kind of polycentric history of psychology that Danziger has outlined.

The book ends with a chapter by Kurt Danziger himself. This chapter contains comments on the chapters by the other authors and also a discussion of some of the issues that these chapters raise. One topic that Danziger explores in some detail is the issue of 'disciplinarity'. Psychologists have traditionally identified their work with the natural sciences and neglected the subject's links to the social sciences and humanities (Danziger, 1994; Brock, 1995a; 1995b). As Danziger points out, the situation is maintained by erecting barriers to subjects like sociology, anthropology, history and philosophy and one possible strategy for changing the situation is to move outside this disciplinary ghetto and to participate in interdisciplinary ventures.

NOTE

REFERENCES

Allport, G. W. (1985). The historical background of modern social psychology. In G. Lindzey and E. Aronson (Eds.), *A handbook of social psychology, vol. 1.* (3rd ed.; pp. 1–46) Cambridge, MA: Addison-Wesley. [Original work published 1954.]

Ash, M. G. (1983). The self-presentation of a discipline: History of psychology in the United States between pedagogy and scholarship. In L. Graham, P. Weingart and W. Lepenies (Eds.), *Functions and uses of disciplinary histories.* (pp. 143–189) Dordrecht: Riedel.

Barbu, Z. (1960). *Problems of historical psychology.* New York: Grove Press.

Berg, J. H. van den (1961). *The changing nature of man.* New York: Norton.

Bhaskar, R. (1978). A realist theory of science. Atlantic Highlands, NJ: Humanities Press.

Bhaskar, R. (1979). The possibility of naturalism. Atlantic Highlands, NJ: Humanities Press.

Boring, E. G. (1950). *A history of experimental psychology.* (2nd ed.) New York: Appleton-Century-Crofts. [Original work published 1929.]

Boring, E. G. (1953). A history of introspection. *Psychological Bulletin, 50,* 169–189.

Brock, A. (1993). Something old, something new: The 'reappraisal' of Wundt in history textbooks. *Theory and Psychology, 3,* 235–242.

Brock, A. (1995a). An interview with Kurt Danziger. *History and Philosophy of Psychology Bulletin, 7,* 10–22.

Brock, A. (1995b). Constructing the subject: An interview with Kurt Danziger. *Psychologie en Maatschappij, 19,* 351–366.

Brock, A. (1998). Pedagogy and research. *The Psychologist, 11,* 169–171.

Bühler, K. (1907). Tatsachen und Probleme zu einer Psychologie der Denkvorgänge: 1. Über Gedanken. *Archiv für die gesamte Psychologie, 9,* 297–365.

Bühler, K. (1908). 2. Über Gedankenzusammenhänge; 3. Über Gedankenerrinerungen. *Archiv für die gesamte Psychologie, 12,* 1–92.

Bühler, K. (1965). *Die Krise der Psychologie.* (3rd ed.) Stuttgart: Gustav Fischer. [Originally published 1927.]

Canguilhem, G. (1955). *La formation de concept du réflexe aux 17e et 18e siècles.* Paris: Presses Universitaire de France.

Danziger, K. (1953). The interaction of hunger and thirst in the rat. *Quarterly Journal of Experimental Psychology, 5,* 10–21.

Danziger, K. (1958). Children's earliest conceptions of economic relationships. *Journal of Social Psychology, 47,* 231–240.

Danziger, K. (1963). Ideology and utopia in South Africa: A methodological contribution to the sociology of knowledge. *British Journal of Sociology, 14,* 59–76.

Danziger, K. (1971). *Socialization.* Harmondsworth: Penguin.

Danziger, K. (1976). *Interpersonal communication.* New York: Pergamon.

Danziger, K. (1979a). The positivist repudiation of Wundt. *Journal of the History of the Behavioral Sciences, 15,* 205–230.

Danziger, K. (1979b). The social origins of modern psychology. In A. R. Buss (Ed.), *Psychology in social context* (pp. 27–45). New York: Irvington.

Danziger, K. (1980a). The history of introspection reconsidered. *Journal of the History of the Behavioral Sciences, 16,* 241–262.

Danziger, K. (1980b). Wundt and the two traditions in psychology. In R. W. Rieber (Ed.), *Wilhelm Wundt and the making of a scientific psychology* (pp. 73–87). New York: Plenum.

Danziger, K. (1980c). Wundt's theory of behavior and volition. In R. W. Rieber (Ed.), *Wilhelm Wundt and the making of a scientific psychology* (pp. 89–115). New York: Plenum.

Danziger, K. (1980d). On the threshold of the new psychology: Situating Wundt and James. In W. G. Bringmann & R. D. Tweney (Eds.), *Wundt studies* (pp. 363–379). Toronto: Hogrefe.

Danziger, K. (1980e). Wundt's psychological experiment in the light of his philosophy of science. *Psychological Research, 42,* 109–122.

Danziger, K. (1983). Origins and basic principles of Wundt's Völkerpsychologie. *British Journal of Social Psychology, 22,* 303–313.

Danziger, K. (1984). Towards a conceptual framework for a critical history of psychology. In H. Carpintero & J. Peiro (Eds.), *Essays in honor of J. Brozek* (pp. 99–107). Valencia: Monografías Rev. Hist. Psicología.

Danziger, K. (1985a). The origins of the psychological experiment as a social institution. *American Psychologist, 40,* 133–140.

Danziger, K. (1985b). The methodological imperative in psychology. *Philosophy of the Social Sciences, 15*, 1–13.

Danziger, K. (1988). Wilhelm Wundt and the emergence of experimental psychology. In G. N. Cantor, J. R. R. Christie, & R. C. Olby (Eds.), *A companion to the history of modern science* (pp. 906–409). Chicago: Chicago University Press.

Danziger, K. (1990). *Constructing the subject: Historical origins of psychological research.* Cambridge/New York: Cambridge University Press.

Danziger (1991). Guest editor's introduction. *History of the Human Sciences, 4*, 327–333.

Danziger, K. (1992a). Ideas and constructions: Reply to reviewers. *Theory and Psychology, 2*, 255–256.

Danziger, K. (1992b). In praise of marginality. Paper presented at the Annual meeting of Cheiron-Europe, Groningen, Netherlands.

Danziger, K. (1993a). Psychological objects, practice, and history. *Annals of Theoretical Psychology, 8*, 15–47.

Danziger, K. (1993b). History, practice and psychological objects: Reply to commentators. *Annals of Theoretical Psychology, 8*, 71–84.

Danziger, K. (1994). Does the history of psychology have a future? *Theory and Psychology, 4*, 467–484.

Danziger, K. (1996). Towards a polycentric history of psychology. Paper presented at the 26th International Congress of Psychology in Montreal.

Danziger, K. (1997a). *Naming the mind: How psychology found its language.* London: Sage.

Danziger, K. (1997b). Essay review: The varieties of social construction. *Theory and Psychology, 7*, 399–416.

Danziger, K. (1999). Natural kinds, human kinds and historicity. In W. Maiers, B. Bayer, B. Duarte Esgalhado, R. Jorna & E. Schraube (Eds.), *Challenges to theoretical psychology* (pp. 78–83). Toronto: Captus Press.

Danziger, K. (2001a). Sealing off the discipline: Wundt and the psychology of memory. In C. D. Green, M. Shore & T. Teo (Eds.), *Psychological thought in the nineteenth century: The transition from philosophy to science and the challenges of uncertainty* (pp. 45–62). Washington: American Psychological Association.

Danziger, K. (2001b). Whither the golden oldies of ESHHS: The historiography of psychological objects. Address at the 20th annual meeting of the European Society for History of the Human Sciences, Amsterdam.

Danziger, K. (2002). How old is psychology, particularly concepts of memory? *History and Philosophy of Psychology, 4*, 1–12.

Danziger, K. (2003). Where history, theory and philosophy meet: The biography of psychological objects. In D. B. Hill & M. J. Kral (Eds.), *About psychology: Essays at the crossroads of history, theory and philosophy.* (pp. 19–33). New York: SUNY Press.

Danziger, K. & Dzinas, K. (1997). How psychology got its variables. *Canadian Psychology, 38*, 43–48.

Daston, L. (Ed.) (2000). *Biographies of scientific objects.* Chicago/London: University of Chicago Press.

Dehue, T. (1998). Community historians and the dilemma of rigor vs. relevance: A comment on Danziger and Van Rappard. *Theory and Psychology, 8*, 653–661.

Draaisma, D. (2000). *Metaphors of memory: A history of ideas about the mind.* Cambridge: Cambridge University Press. [Originally published in Dutch in 1995.]

Driesch, H. (1926). *Grundprobleme der Psychologie: Ihre Krisis in der Gegenwart.* Leipzig: Reinicke.

Ebbinghaus, H. (1885). *Über das Gedächtnis.* Leipzig: Duncker und Humblot.

Gergen, K. (1985). The social constructionist movement in modern psychology. *American Psychologist, 40*, 266–275.

Hacking, I. (1995). The looping effects of human kinds. In D. Sperber, D. Premack & A. J. Premack (Eds.), *Causal cognition: A multi-disciplinary approach* (pp. 351–383). Oxford: Clarendon Press.

Harré, R. (1983). *Personal being: A theory for individual psychology.* Oxford: Blackwell.

Heidbreder, E. (1933). Seven psychologies. New York: Century.

Loewenstein, B. (Ed.) (1992). *Geschichte und Psychologie: Annäherungsversuche.* Pfaffenweiler: Centaurus.

Mannheim, K. (1936). *Ideology and utopia: An introduction to the sociology of knowledge*. London: Routledge.

Moghaddam, F. M. (1987). Psychology in the three worlds as reflected by the crisis in social psychology and the move toward indigenous third-world psychology. *American Psychologist, 42*, 912–920.

Morsbach, H. & Tylor, W. J. (1976). Some Japanese-Western linguistic differences concerning dependency need: The case of amae. In R. Harré (Ed.), Life sentences: *Aspects of the social role of language*. New York: Wiley.

Nelson, R. D. (1992). The analysis of styles of thought. *British Journal of Sociology, 43*, 25–54.

Peeters, H. (1996). *Psychology: The historical dimension.* Tilburg: Syntax.

Richards, G. (2002a). History of psychology: The discipline of the future. *History and Philosophy of Psychology, 4*, 13–22.

Richards, G. (2002b). *Putting psychology in its place: A critical historical overview*. (2nd ed.) London: Routledge.

Selye, H. (1978). *The stress of life*. New York: McGraw-Hill.

Smith, R. (1992). *Inhibition: History and meaning in the sciences of mind and brain.* Berkeley/Los Angeles: University of California Press.

Sonntag, M. & Jütteman, G. (Eds.) (1993). *Individuum und Geschichte: Beiträge zur Diskussion um eine "Historische Psychologie"*. Heidelberg: Asanger.

Staeuble, I. (1991). 'Psychological man' and human subjectivity in historical perspective. *History of the Human Sciences, 4*, 417–432.

Staeuble, I. (1993). History and the psychological imagination. *Annals of Theoretical Psychology, 8*, 85–117.

Van Rappard, J. F. H. & van Strien, P. J. (1993). History ands theory: Introduction. *Annals of Theoretical Psychology, 8*, 1–14.

Van Rappard, J. F. H. (1997). History of psychology turned inside(r) out: A comment on Danziger. *Theory and Psychology, 7*, 101–105.

Van Rappard, J. F. H. (1998). Towards household history: A reply to Dehue. *Theory and Psychology, 8*, 663–667.

Verhave, T. & van Hoorn, W. (1984). The temporalization of the self. In K. J. Gergen and M. M. Gergen (Eds.), *Historical social psychology* (pp. 325–345). Hillsdale, NJ: Erlbaum.

Vygotsky, L. [Wygotski, L.] (1985). Die Krise der Psychologie in ihrer historischen Bedeutung. In *Ausgewählte Schriften I* (pp. 57–277). Berlin: Volk und Wissen. [Written in Russian in 1926 and originally published in this language in 1982.]

Winston, A. S. & Blais, D. J. (1996). What counts as an experiment: A transdisciplinary analysis of textbooks, 1930–1970. *American Journal of Psychology, 109*, 599–616.

Woodworth, R. S. (1931). *Contemporary schools of psychology.* New York: Ronald Press.

RECONSTRUCTING THE SUBJECT

KURT DANZIGER AND THE REVISIONIST PROJECT IN HISTORIOGRAPHIES OF PSYCHOLOGY[1]

HENDERIKUS J. STAM

> The greatest obstacles to good scholarship are to be found in the 'god tricks' that serve to hide and obscure the necessary partiality involved in knowledge production.
>
> Kurt Danziger (1998)

INTRODUCTION

It is both a pleasure and a privilege to contribute to this volume of scholars honoring the work of Professor Kurt Danziger. His influence is deeply felt by all of us who have attempted to understand the history of the discipline of psychology as more than a mere accumulation of ideas and empirical results and he has inspired the critical work of those who have attempted to change the mainstream of that discipline by showing us that investigative practices are deeply carved out of taken for granted worlds.

I would like here especially to consider the relationship between Danziger's work and questions of theory. This is a tall order indeed so I intend to engage only a modest aspect of that topic here, namely the epistemological implications for psychological theory embedded in the historical work conducted by Danziger. The continuous interplay between our theoretical discourse and investigative practices and the embeddedness of those practices in social contexts not of our own making makes psychological theory the outcome of more than the ideas of individual

scientists. Addressing this topic should be relatively effortless given that Danziger has himself addressed the implications of his work in both of his major historical books, *Constructing the Subject* (1990a) and *Naming the Mind* (1997a) as well as in numerous other papers (e.g., Danziger, 1990b, 1993a, 1994). On the other hand, having addressed the issue he has also shown that it is not a simple matter of drawing out a few implications from his work but that on his own account history and theory are clearly not independent activities. In this sense Danziger can be counted among those who, like others such as Michel Foucault, have challenged the prevailing ethos of the human sciences and argued that from the vantage of history there is so much more at stake in these sciences than was at first supposed.

By way of introducing the narrative, the point of my paper is perhaps best captured by the following anecdote: While attending a conference in 1989 at which Danziger gave a talk in which he outlined some of his key notions in the history of psychology prior to the publication of *Constructing the Subject* (Danziger, 1990a), I was seated beside a senior psychologist. During the ensuing discussion, which consisted mainly of a rather predictable debate on the distinction between intellectual and contextual history, Danziger held his ground without allowing himself to be drawn into the more exaggerated and heated aspects of the contest. My senior colleague turned to me and pronounced that he wished Danziger would take a stronger stand because "we need our historians to provide us with a vision." Presumably my colleague meant a vision of what the discipline *could* be in the light of the kind of critical history Danziger has written. In retrospect however I do believe that there is a vision in the work of Kurt Danziger, and it is that vision that I would like to place on the table. For in elaborating this particular aspect of Danziger's work we will come closer to addressing the question of history and its relationship to theory.

THEORY

Theory, from the Greek $\theta\varepsilon\omega\rho ia$ and Latin *theōria* meant, among other things, contemplation or observation. This meaning has lingered in the modern English usage of the term; as late as 1710 John Norris could say that "speculative knowledge contemplates truth for itself, and accordingly stops and rests in the contemplation of it, which is what we commonly call theory" (as cited in the *OED*). At the same time of course we see the gradual adoption of the term within the sciences and its strict application by Newton to mean "invariant relations among terms designating manifest qualities" (Losee, 1980, p. 91). He divided this meaning strictly from his views on 'hypotheses' which are statements about terms for which no measuring procedures are known (hence Newton's famous dictum, "Hypotheses non fingo"). This view was modified over a period of 300 years up to the logical empiricists of the twentieth century who claimed that theories must be deductive systems in

which laws are theorems. The spectacular demise of the logical empiricist system in the space of forty years has been widely described and analyzed and I will not pursue this here. In short, it is the failure (or impossibility) of maintaining the key distinction between a theoretical language and observational terms that created such difficulties both for logical empiricists and for those who would formalize theory in science more generally.

Theory in the human sciences, and in psychology in particular, never approximated the grand schemes articulated by luminaries of the logical empiricist movement such as Hempel and Carnap. Nonetheless, the latter provided a kind of framework outside the discipline that could be called on at auspicious moments for defense. Logical empiricism worked as a kind of Non-proliferation Treaty for theory, where theory could be contained so long as it was held to be, in principle, a species of deductive system. However the notion that observations were dependent on, continually infected by, or otherwise structured by theoretical considerations (e.g., Hanson, 1958; Kuhn, 1970) opened up the question of theory in the philosophy of science more generally and eventually did so in psychology (e.g., Stam, 1996). The obverse is frequently left unsaid, namely that theory is deeply dependent on some presumed observational regularities of life itself, even in its most post-positivist moments. That is, theorizing and observing are not different kinds of activities so much as they are different forms of a similar activity of sense-making that varies in its systematicity, practical arrangements and consequences (Stam, 2000). In this the writing of history is not different in so far as it requires the adjudication of evidence always within a framework of prejudices and preconceptions, theoretical predilections and considerations of what audience one expects to address.

HISTORY OF PSYCHOLOGY

How do these considerations inform a discussion of the historical work of Kurt Danziger? Historiography is obviously deeply dependent on some presuppositions and pretexts that allow the enterprise to establish its legitimacy. The difficulties of the philosophy of history notwithstanding, the peculiar nature of disciplinary history only compounds such difficulties. For not only are we confronted with the question of what constitutes proper historical inquiry or 'explanation' but also with what constitutes the discipline in question, in Danziger's case, psychology. These are not trivial problems, for while we may have agreement on how to proceed in writing a history of psychology we may not agree on what ought to constitute psychology, or vice versa. Fortunately, it turns out that these questions are related so that the answer to one is at least affected if not inspired by one's answer to the second. This relationship exists on several levels: First, any historical undertaking is concerned with the activities, artifacts, expressions and desires of human beings

and human collectives. Such conceptions of human nature that the historian has, and that are predominant in the discipline and culture of the historian, must obviously influence the work at hand, or on some accounts of history, make the work possible in the first instance. Second, the disciplinary history of psychology is also, in part, a history of the activities and artifacts of human beings, namely psychologists. Hence, the historian of psychology is first of all a historian, piecing together a narrative or account of places, persons, desires, contexts and ideas.

Nevertheless, if our conception of human persons is like that of the mainstream of the discipline, that is, largely scientistic, individualistic and functional, then our history will focus on the development of disciplinary achievements and not on the institutional, political, social or even depth-psychological forces involved in creating such a model of human being in the first place. Or, if it is our primary goal to tell a story of the rise and development of aspects of the discipline proper, it is likely that we will remain confined to a disciplinary trajectory.

To illustrate, I want briefly to examine critical history's perennial foil, Edwin Boring.[2] The first sentences of the 1929 edition of his text are,

> The history of psychology is inextricably bound up with the history of philosophy, whereas the rise and development of experimental psychology is explicitly a phase of the history of scientific endeavor. (p. 3)

As Boring himself noted later in his memoirs, he wrote his history of psychology out of a conception of history as progress. He noted that "...History is an ever-flowing stream through the centuries, a stream of events that occur in the nervous systems of persons situated so that their thoughts and acts become links in the course of progress" (Boring, 1961, p. 49). Such presentist history has long been criticized and the obvious variants still available today in the form of some under-graduate text books only speak to the lasting importance of such historiography to a discipline without a center, still uncertain about its scientific and institutional status.

As soon as notions such as "progress" and the very history of science itself become contested, however, then the writing of such a disciplinary history becomes a matter for revision. And it is here that I would place Professor Danziger among the foremost practitioners of this revisionist history in psychology. Inspired by new histories of science and the social studies of science, it was possible to confront the seemingly ironclad notion of the division between an internal reconstruction of science and its external reconstruction. The theories of scientists are generated in the *activities* of scientists that are conducted in social institutions that have at least some of the characteristics of other, non-scientific social institutions. Knowing the historical location and specificity of our activities ought to be, on this account, a normal part of the understanding of generating theory and research.

Indeed, both the understanding of the history of a problem and the construction of its theory are, on this account, not radically different activities. Among

other things, history makes it possible to ask what the relationship is between one's interest as a scientist, one's membership in a particular local community and one's accounts of one's scientific activities. For example, the relationship between ideas of intelligence and intellect, the institutionalization and universalization of education, and the grading of human abilities all played a role in the development of the intelligence test in a manner that complicates any story of heroic pioneers who developed such tests (e.g., Danziger, 1997a). The post-war institutionalization of North American experimental social psychology can hardly be conceived along the lines of brilliant individuals applying a new technology to a whole new field of human experience using selected insights garnered from Kurt Lewin. Instead, a complex relationship exists between ambitious post-war psychologists (who largely came from working-class urban environments), the need to demarcate social psychology from other fields of endeavor inside and outside psychology, and the recognition that only a psychology based on individuals would ever survive as social psychology inside the discipline. Along with more idiosyncratic contributions of the individuals involved, this provides a more coherent and context-sensitive account of the development (and failures) of contemporary social psychology (e.g., Stam, Lubek & Radtke, 2000). The point is not that the history of intellectual endeavors is complex but that the history of such endeavors are always unfolding and open to further contextualization and elaboration and can never *just* be accounts of intellectual achievements.

As an aside, note that the focus on the formative activities of scientists in laboratories and their relation to theory are also a consequence of the underdetermination of theories by data. That is, in the absence of genuine epistemological authority, most notably within the human sciences (see e.g., Weimer, 1979), scientists must retreat to sophistication and commitment as well as traditions of investigation. But such a retreat is negligible if the practical activities of one's scientific activities are determined to be progressive by the community of science or one is able to demonstrate technological achievements to the world at large. In the absence of both, the activities of psychologists are important not only for what they reveal of the construction of a discipline but for what they hide. That is, psychological theories both permit and pretermit, precisely what a historical recounting of the activities of psychologists ought to open up to view. History makes visible not just the obvious but the hidden interrelated processes of constructing and then separating data and theory out of a world of artifacts.

In *Constructing the Subject* (1990a) Danziger demonstrates how a treatment of the investigative practices of the discipline radically shifts the emphasis of disciplinary history. Rather than tracking a unitary conception of the discipline, the question of investigative practices makes clear how psychology became a hybrid of various technologies of investigation. Together with a division of labor in the laboratory, the careful development of markets for its knowledge and the incorporation of statistical devices into its methods and manner of theorizing in the

aggregate, psychology's history no longer resembled the kind of linear, incremental enterprise we had come to expect from histories of psychology. In a later paper Danziger (1993a) extends this analysis to the "historicity of psychological objects" or the very things to which our theories refer. Here he reminds us that our objects of investigation are constructed, that is, they are the product of human activities, they have definitive uses, and they have a reference that itself needs to be explicated. This paper on the historicity of psychological objects seems to me a transitional one, pointing to a need for further analysis that was left open by the use of the term "objects." The latter have the status of kinds of 'hybrid' entities (or quasi-objects, cf. Latour, 1993) that are at once natural and social, material and discursive. Latour's claim is that distinctions between 'constructed' and 'material' accounts are misguided, all of our research objects have the character of both material and discursive, socially mediated properties. The construction and proliferation of such hybrids is not just the outcome of an investigative practice but includes the reformulation of powerful linguistic resources as well.

It seems that this problem is addressed in *Naming the Mind* (1997a). In this volume the project is extended to the level of concept and terminology (rather than investigative practices or their objects). Indeed, by not just focusing on theory or strictly formal expositions, Danziger is able to keep from lapsing into old debates on the nature of psychological categories. Instead he argues that the very act of categorization in psychology displays a naive naturalism whereby natural kinds are presumed to exist in the categories that make up the theories of psychology. Yet by the time these theories are articulated in a formal sense, the act of naming and pointing to the appropriate object of investigation has already smuggled in a host of presuppositions and assumptions. Terms such as intelligence, emotion, motivation and the like are neither neutral nor natural but carry histories of conceptualization and use that deeply influence the possibilities open to the psychological theory that uses the concept. As Danziger notes, some of our most important terms are scientized and institutionalized variants of an eighteenth-century moral language.

Naming the Mind completes the earlier study of the investigative practices of psychologists in *Constructing the Subject* by combining this work with a categorical and discursive study. Danziger's argument shifts from the crucial role played by investigative practices to the language guiding and in turn produced by those practices. As I will discuss below, this shift is important for the way Danziger has come to see the shaping of the discipline and the importance that a psychological discourse has above and beyond the research practices of its members. In short, Danziger argues that psychology established itself institutionally through astutely combining universal biological meanings with local social meanings that were mediated by the development of specific technologies. Certain investigative practices, certain methods of psychological research and assessment—intelligence and personality testing, techniques for measuring the strength of attitudes and motives, standard learning situations, and so on—provided the basis for constituting classes

of scientifically validated phenomena that could be produced in a variety of practical settings. In the course of time, the role of such technologies in establishing the meaning of psychological categories became ever more decisive.

At this point we have come full circle, for the investigative practices are implicated in, and part of the discursive structure of the discipline. To return to my earlier formulation, it is here that theorizing and observing are indeed activities that are not separate but in their mutual organization and maintenance come to constitute disciplinary practices and findings. To stay with the visual trope, both observing and theorizing are attempts to make visible that which is conceived of as invisible and to render invisible or subsidiary other, competing accounts of the subject matter. The creation of objects of investigation and the findings related to those objects are rhetorical accomplishments as well as moments of invention. It is not only the language of psychology that is changed and shaped by the constitution of these objects but it is practices that are made possible by the objects in their creation.

Narrating history in this manner leaves open a question that Danziger himself has worried about in his work across the span of two decades, namely, what are the implications for the current enterprise of psychology, or as he asked in *Constructing the Subject*, "when allowance is made for the factors that led to a relativizing of psychological knowledge, is there no remainder?" (p. 192). I will return here to an earlier worry that I noted in my review of *Constructing the Subject* (Stam, 1992), but with an intervening decade to consider the problem I would like to take a slightly different approach to this question. I was originally concerned that in that book, Danziger had backed out of the implications of his own analysis by noting that psychological realities could not be entirely accounted for by the limits of their investigative contexts and remained hidden under a veil of socially constituted practices. Like some of Danziger's critics (e.g., Ash, 1993; Mills, 1993) there is a continuing worry that something is being glossed or overlooked and that that *something* in fact consists of the core phenomena of the discipline.

Danziger's answer to this was initially to call on a form of critical realism as a solution (Bhaskar, 1978). The domain of the real was distinguished from the domain of the actual, on Bhaskar's account, and the possibility was held up that there are determinant psychic mechanisms responsible for, or underlying, the observed regularities constituted through the investigative practices of psychologists. However, Danziger himself has moderated these claims on the realist-relativist question in his further work. For example, in his 1993 paper Danziger argues that psychology's objects are not natural kinds and that methods are not theoretically and ethically neutral. Instead, argues Danziger, theories ought to be evaluated on criteria of practical consequences and reflexivity. By the time of the publication of *Naming the Mind*, this has retreated even further to the background. Here Danziger refers not to 'objects' but to the problematic relationship between discursive categories and the phenomena themselves. Danziger clearly notes that the relationship

here is *constitutional*, not representational, by which he means that a psychological object depends on "its human creator and the relationship between the object's existence and its representation has become quite intimate" (p. 187). Relying on Ian Hacking's notion of a 'human kind' as opposed to a 'natural kind,' Danziger notes that psychological objects aren't just legends either. They have a circulation (in Hacking's words [1994] they are subject to "looping effects") in a cultural and human context and their circulation amends as well as reifies the phenomena in question.

WHAT CAN HISTORY BE?

Although his critics have accused him of, among other things, being a sociological reductionist (Mills, 1993) or of denigrating the possibilities of writing a history from the 'inside' of the discipline (Rappard, 1997)[3], I think these critiques are off the mark (and Danziger has spoken eloquently for himself in reply, e.g., Danziger, 1993b, 1997b, 1998). I would like to place my comments in the context of broader debates in the philosophy of history. This is because the critiques of the work of historians such as Danziger are often couched in terms of the pernicious effects of relativism and explicitly or implicitly are aimed at propping up some conception of realism (e.g., Fox-Genovese & Lasch-Quinn, 1999). In his mature writings, it was R. G. Collingwood who recognized the mistake in this for the enterprise of history. Often accused of skepticism himself by reviewers of *The Idea of History* (1946), Collingwood was careful not to become mired in this debate. For after all, in his earlier works such as *Speculum Mentis* (1924) he endorsed a realist program for history, if only implicitly, and by the time the *Idea of History* was published he had worked out precisely why he was not a realist. Skepticism, he argues, is a consequence of realism, "the discovery that the past as such is unknowable is the skepticism which is the permanent and necessary counterpart of the plain man's realism" (1965, p. 100). It is the search for a factual past that is an illusion because the past as such can never be known again. Instead it led Collingwood away from "an unknowable past-in-itself" to the activities of historians themselves (Goldstein, 1970). Here Collingwood is often seen as relegating history to an act of imagination but this is too quick: In a paper on the historical imagination appended to the *Idea of History* he argues,

> ... neither the raw material of historical knowledge, the detail of the here-and-now as given him in perception, nor the various endowments that serve him as aids to interpreting this evidence, can give the historian his criterion of historical truth. That criterion is the idea of history itself: the idea of an imaginary picture of the past. That idea is, in Cartesian language, innate; in Kantian language, *a priori*. It is not a chance product of psychological causes; it is an idea which every man possesses as part of the furniture of his mind, and discovers himself to possess in so far as he becomes conscious of what it is to have a mind. (1946, p. 248)

What keeps the "self-dependent, self-determining, and self-justifying" (p. 249) historical imagination from falling into skepticism is the discipline of history itself. Although Collingwood was not entirely clear about this, it is the structure of the discipline and what this discipline considers as good research practices, reliable evidence and the like that prevents the individual knower/historian from sliding off into the *mere* play of imagination. And Collingwood defended the notion of the autonomy of history precisely to preserve its status as a communal enterprise (Goldstein, 1970).[4]

Collingwood saves history from the endless spiral of skepticism by an explicit turn to the imagination or the psychology of the individual historian. By extension, the community of historians makes history possible outside of any other authority. In this manner, Collingwood sees clearly that it is in its communal activities that historians decide history. This formulation predates the work of others who take up the problem of narrative, plot, and understanding, in particular Paul Ricoeur and Hayden White. The latter become preoccupied with a question of how language in its myriad forms makes the structure of story possible but when Ricoeur argues (against positivist textual objectivity) for a dialectic of understanding and explanation he means, in a manner reminiscent of Collingwood, that understanding is the ability to take up again, within the self, the work of structuring that is performed by the text. Explanation is always secondary to this understanding in that it consists in bringing to light the codes underlying the work of structuring. It is clear that understanding for Ricoeur is an imaginal act and explanation is made possible by the discourse available to us from our cultural understanding and presuppositions. History must be configured and brought into meaningful relation with other events in time, that is, made subject to emplotment.

In the first volume of *Time and Narrative* Ricoeur (1984) attends to the necessity of narrative (through configuration and emplotment) for a historical understanding. Indeed, history must be configured, it must be brought into a meaningful relation with other events in time. Like Hayden White, Ricoeur argues that history is combined of the found and made-up, of the documented and the narrated. Without configuration and emplotment there can be no history. Once narrated history is appropriated it is not only meaningful but it in turn becomes the ground for further configuration. Narratives extend the past into the present and make it possible to imagine a future (Ricoeur, 1988).

Like Ricoeur, a range of historians of the twentieth-century has taken up the problem of the relation between the found and the made-up or the documented and the narrated. Hayden White (1973, 1978) too has argued that to produce a history, the chronicle must be converted to a meaningful narrative and hence must be emplotted. But the past has no plot and hence the historian provides an account, a narrative that emplots or encodes the traces or evidence. White is more formalist than Ricoeur (and other narrative historians), however, in so far as he argues that modes of emplotment are fundamentally dependent on tropes since there is no other entry into the rhetorical structure of language. Indeed, figurative characterizations

are presupposed by the events to be represented and hence White's claim that language operates tropologically to prefigure a field of perception. The boundary between the language that makes history and the content of that history remains always opaque.

Despite White's move into the formalism, so characteristic of later 20th century theorizing, he effectively supports Collingwood's contention that history is an independent enterprise (even as he does not support Collingwood's notion that history requires the reenactment of historical agents). All such theories of history are meant to prevent the encroachment of positivism and scientism on a historical consciousness. They embody the insights characteristic of the *Geisteswisschenschaften* debates of the late 19th century where Dilthey already formulated the notion that lived experience is mediated through the imagination as well as the socio-cultural practices of the historical world (Makkreel, 1992; Mos, 1996).

It was Ankersmit (2002) who recently argued that historical representation is a matter of the organization of the truth rather than the truth itself. Our representations may be "sensible, fruitful, helpful, thought-provoking (or not), but, while the data deployed may be true or false, the proposal deploying them cannot be" (p. 38). Hence the criteria are broadly aesthetic; there is no direct line back to the agents of history except through another point of view. But this point of view is not in the past but is embedded in the aesthetic language of the historian (Ankersmit, 1996). On this account history takes its force precisely from the need to represent the past in the absence of a fixed algorithmic manner of moving from the past to writing about that past (see also Stam, 2003).

Attempts to retain for history its privileged capacity to judge the past are rare and today they exercise mostly those who see themselves as defenders of some version of 'objectivity' in the self-styled culture wars whose battles seem to be largely confined to university campuses in the United States (see Fox-Genovese & Lasch-Quinn, 1999). To return to Danziger's work on psychology then, in the context of the larger debates in the philosophy of history Danziger's work is not nearly as controversial or threatening as it appears to psychologists. But precisely because his audience has consisted largely of psychologists (and it would be a mistake not to write for psychologists), Danziger finds himself on the defensive for reasons that might seem odd to professional historians. Part of this is due to the role that histories of psychology have traditionally played in the discipline.

Historical studies of psychology are first and foremost histories. Nonetheless, as Danziger has already pointed out on several occasions, their traditional function was to serve a pedagogical role within the discipline (e.g., Danziger, 1994). This function precludes historical studies from contributing to psychology as a disciplinary project and constitutes a mobilization of the tradition for the purposes of celebrating the accomplishments of the past and justifying the present. Such representations of the past are premised on the continuity of the present. On an aesthetic reading such histories are the least interesting and most conventional. They reflect

the discipline as we have come to understand it, without in any way illuminating the subject matter of the discipline, namely the nature of human psychology itself.

What Danziger has demonstrated with his histories of psychology is a way of proceeding that allows us to turn history on the subject matter of psychology itself. In this respect psychological studies are always historical; they reflect the formalization of language and the development of techniques that emerge out of our shared cultural goods. In that sense they do not entirely escape their origins in particular life-worlds. For even when we apply such routine tools as statistics to our psychological topics we do not escape a concern with number, efficiency, normativity and so on that are entailed in such devices.[5]

CONCLUSIONS

Disciplinary histories are specialized forms of history but history nonetheless. What Danziger's work makes so clear with respect to Collingwood's claim that it is the community of historians that ultimately regulates the work of the historian, is that likewise, it is the community of psychologists that regulates the work of the psychologist. What we do not know is, which discipline is to be regulative for the *history* of psychology. It is here that we can see the argument most clearly, for it is those who are wedded to a progressivist or positivist notion of history that see a limited role for that history and wish that history to be on bended knee before the *scientific* authority of psychology.[6] But the respect and authority of science can never be granted to a historical account of it, even if that history is merely 'celebratory' or presentist. For history cannot be science, in the same way that history is never *just* literature. It is here that psychology and history come together, for in order to know what the institution of psychology *is* we must have a history of it as it has been practiced. Yet the *history* of psychology already presupposes that we know what psychology in fact is. Hence the inseparability of the enterprise of determining the subject matter of psychology from its history. The story we tell about psychology is always both a historical and an implicitly teleological one.

Critical historians of psychology have shifted their allegiance and they are no longer beholden to the scientific claims of the discipline. After all, these are exactly what need to be understood again from a historical perspective. Their regulative community exists elsewhere, in the history of science, within the community of critical psychologists, and so on. Hence their histories contribute theory to different communities with different sensibilities and criteria for knowledge. It is not that these communities necessarily speak incommensurate languages, but there are recognizable differences. It is Professor Danziger who is among the very best of those who have shown us that the picture of paradise created by traditional psychological histories was illusory and having tasted the forbidden fruit of critical

historical knowledge there is no return from the exile in which we find ourselves. The vision in Danziger's work then consists of a discipline that is no longer fettered to the chains of an epistemology that constricts our theoretical claims at every turn. In his own words,

> . . . changes in psychological categories will continue to be heavily dependent on changes in the societies within which these categories have a role. Their meaning will continue to be negotiated and contested among the groups to whom they matter. (Danziger, 1997a, p. 193)

To end where I began, I would like to close this chapter with another anecdote: several years ago I attended a conference in Canada and was engaged in conversation by a retired colleague from a western Canadian university. He asked me to recommend some historical works on a particular topic and, as luck would have it, he just happened upon a topic that allowed me to rattle off a series of book titles. Impressed, he inquired, "Weren't you a graduate student of Kurt Danziger's?" I had to disappoint him and told him that no, I had studied with the late Nick Spanos, who although having had historical interests, was better known for his critical work on hypnosis and multiple personality. My colleague seemed disappointed but I took it as a compliment. I can only hope that Professor Danziger takes it the same way.

NOTES

[1] Department of Psychology, University of Calgary, Calgary, Alberta, Canada T2N 1N4. An earlier version of this paper was presented at a symposium in honor of Kurt Danziger at the European Society for the History of the Human Sciences meetings, Berlin, Germany, August 2000. I thank Adrian Brock and the organizers of that symposium for inviting me to participate and I am grateful to Kurt Danziger and the Editors for their generous comments on earlier drafts.

[2] I should add here that I do not wish to denigrate Boring's contributions to the institutional development of the history of psychology, especially with regard to the important role he played in legitimating historical studies as a pursuit *within* psychology. Boring could also be ambivalent in his presentism: "a psychological sophistication that contains no component of historical orientation seems to me to be no sophistication at all" (1929, p. vii).

[3] Or even of criticizing "celebratory" accounts in favor of "condemnatory" accounts (Dehue, 1998).

[4] Connelly and Costal (2000) have recently argued that Collingwood's ideas on history also contained a version of a historical psychology that remains largely unelaborated.

[5] One reviewer of this chapter noted that this and other descriptions makes it appear that Danziger's work has something in common with the French Annales school which formed around Fernand Braudel in the 1950s and 60s. Known for its 'total' approach to history, there were no details of daily life too large or too small to contribute to historical accounts (often called 'social history'). Braudel was famous for wishing to break down the boundaries of the social sciences in the name of an 'interscience.' Nonetheless, Danziger does not share the school's penchant for economic explanations and the need for structural accounts, however sophisticated. Furthermore, intellectual work must always be more than the product of economic and social history since it is constituted in an international discourse that is continually contested across large geographical, social and economic domains.

[6] Kendler's (1987) textbook is perhaps one of the clearest examples of this.

REFERENCES

Ankersmit, F. R. (1996). *Aesthetic politics: Political philosophy beyond fact and value.* Stanford: Stanford University Press.

Ankersmit, F. R. (2002). Representational democracy: An aesthetic approach to conflict and compromise. *Common Knowledge, 8*(1), 24–46.

Ash, M. G. (1993). Rhetoric, society, and the historiography of psychology. In H. V. Rappard, P. van Strien, L. P. Mos & W. J. Baker (Eds.), *Annals of Theoretical Psychology, Vol. 8* (pp. 49–57). New York: Plenum.

Bhaskar, R. (1978). *A realist theory of science*. Atlantic Highlands, NJ: Humanities Press.

Boring, E. G. (1929). *A history of experimental psychology*. New York: Century Co.

Boring, E. G. (1961). *Psychologist at large*. New York: Basic Books.

Collingwood, R. G. (1924). *Speculum mentis*. Oxford: Clarendon.

Collingwood, R. G. (1946). *The idea of history*. Oxford: Clarendon.

Collingwood, R. G. (1965). *Essays in the philosophy of history*, edited with an introduction by W. Debbins. Austin, Tex.: University of Texas Press.

Connelly, J. & Costal, A. (2000). R. G. Collingwood and the idea of a historical psychology. *Theory & Psychology, 10*, 147–170.

Danziger, K. (1990a). *Constructing the subject: Historical origins of psychological research*. Cambridge: Cambridge University Press.

Danziger, K. (1990b). The social context of research practice and the history of psychology. In W. J. Baker, M. E. Hyland, R. van Hezewijk & S. Terwee (Eds.), *Recent trends in theoretical psychology, Vol. II* (pp. 297–303). New York: Springer-Verlag.

Danziger, K. (1993a). Psychological objects, practice, and history. In H. V. Rappard, P. van Strien, L. P. Mos & W. J. Baker (Eds.), *Annals of Theoretical Psychology, Vol. 8* (pp. 15–47). New York: Plenum.

Danziger, K. (1993b). History, practice, and psychological objects: Reply to commentators. In H. V. Rappard, P. van Strien, L. P. Mos & W. J. Baker (Eds.), *Annals of Theoretical Psychology, Vol. 8* (pp. 71–84). New York: Plenum.

Danziger, K. (1994). Does the history of psychology have a future? *Theory & Psychology, 4*, 467–484.

Danziger, K. (1997a). *Naming the mind: How psychology found its language*. London: Sage Publications.

Danziger, K. (1997b). The future of psychology's history is not its past: A reply to Rappard. *Theory & Psychology, 7*, 107–111.

Danziger, K. (1998). On historical scholarship: A reply to Dehue. *Theory & Psychology, 8*, 669–671.

Dehue, T. (1998). Community historians and the dilemma of rigor vs relevance: A comment on Danziger and Van Rappard. *Theory & Psychology, 8*, 653–661.

Fox-Genovese, E. & Lasch-Quinn, E. (Eds.) (1999). *Reconstructing history*. New York: Routledge.

Goldstein, L. J. (1970). Collingwood's theory of historical knowing. *History and Theory, 9*, 3–36.

Hacking, I. (1994). The looping effects of human kinds. In D. Sperber, D. Premack & A. J. Premack (Eds.), *Causal cognition: A multi-disciplinary approach* (pp. 351–383). Oxford: Clarendon Press.

Hanson, N. R. (1958). *Patterns of discovery*. Cambridge: Cambridge University Press.

Kendler, H. H. (1987). *Historical foundations of modern psychology*. Pacific Grove, CA: Brooks/Cole.

Kuhn, T. S. (1970). *The structure of scientific revolutions*, 2nd ed. Chicago: University of Chicago Press.

Latour, B. (1993). *We have never been modern*. Cambridge, Mass.: Harvard University Press.

Losee, J. (1980). *A historical introduction to the philosophy of science*, 2nd ed. Oxford: Oxford University Press.

Makkreel, R. A. (1992). *Dilthey: Philosopher of the human studies*. Princeton, N.J. Princeton University Press.

Mills, J. (1993). Contextualizing Danziger within sociological theory. In H. V. Rappard, P. van Strien, L. P. Mos & W. J. Baker (Eds.), *Annals of Theoretical Psychology, Vol. 8* (pp. 65–70). New York: Plenum.

Mos, L. P. (1996). Immanent critique of experience: Dilthey's hermeneutics. In C. Tolman, F. Cherry, R. van Hezewijk, & I. Lubek (Eds.), *Problems of theoretical psychology* (pp. 368–377). Toronto: Captus.

Rappard, J. F. H. van (1997). History of psychology turned inside(r) out: A comment on Danziger. *Theory & Psychology, 7*, 101–105.

Ricoeur, P. (1984). *Time and narrative*, Vol. 1. (K. McLaughlin & D. Pellauer, trans.). Chicago: University of Chicago Press.

Ricoeur, P. (1988). *Time and narrative*, Vol. 3. (K. Blamey & D. Pellauer, trans.). Chicago: University of Chicago Press.

Stam, H. J. (1992). Deconstructing the subject: Banishing the Ghost of Boring. [Review of *Constructing the subject: Historical origins of psychological research*]. *Contemporary Psychology, 37*, 629–632.

Stam, H. J. (1996). Theory & practice. In C. Tolman, F. Cherry, R. v. Hezewijk, & I. Lubek (Eds.), *Problems of Theoretical Psychology* (pp. 24–32). Toronto: Captus Press.

Stam, H. J. (2000). Theoretical Psychology. In K. Pawlik & M. R. Rosenzweig (Eds.), *International Handbook of Psychology* (pp. 551–569). London: Sage.

Stam, H. J. (2003). Retrieving the past for the future: Boundary maintenance in historical and theoretical psychology. In D. B. Hill and M. J. Krall (Eds.), *About psychology: Essays at the crossroads of history, theory, and philosophy* (pp. 147–163). New York: SUNY Press.

Stam, H. J., Lubek, I. & Radtke, H. L. (2000). Strains in experimental social psychology: A textual analysis of the development of experimentation in social psychology. *Journal of the History of the Behavioral Sciences, 36*, 365–382.

Weimer, W. B. (1979). *Notes on the methodology of scientific research*. Hillsdale, N.J.: Erlbaum.

White, H. (1973). *Metahistory*. Baltimore: John Hopkins University Press.

White, H. (1978). *Tropics of discourse*. Baltimore: John Hopkins University Press.

IN SEARCH OF METHOD

Johann Louw

INTRODUCTION

Since the early 1980s the historiography of psychology has undergone a significant transformation. The social contextualization of the history of psychology has been a defining component of this change, the acknowledgement of and search for the historical roots of psychological knowledge in specific social settings. One of the first publications to explore and plead for a recognition of the social origins of modern psychology was the edited book, *Psychology in Social Context* (Buss, 1979). The title of this volume, and the aims outlined in its opening chapter, signal its debt to the sociology of knowledge. Buss stated that

> Psychology as practiced by professional academicians occurs within a social context; psychological knowledge is tied to the infrastructure of a society of socially defined groups. (p. 2)

As a social activity, the construction of knowledge also has a historical dimension:

> To properly understand and evaluate the validity of ideas, theories, and concepts of psychology, one must adopt a sociohistoric interpretation. (p. ix)

Thus psychology had to pay attention to its social basis, and had to acknowledge that external forces had an impact on internal developments in the discipline.

In this essay I wish to return to the influence of the sociology of knowledge on these early developments. I will argue that this tradition can still be recognized in current debates, even if it is just in the recognition of overtones of constructivist epistemologies in them. Certainly, the "contextualist" analysis of psychological concepts and methods extends the tradition in some versions of

social constructionism. The work of Kurt Danziger has played no small part in this process, and his chapter in Buss (1979) forms a pivotal transition point in his own work on the history of psychology. Indeed, his curriculum vitae shows a clear break around this time: he published this chapter (1979a), and "The positivist repudiation of Wundt" (1979b), and since then has published only in the history of psychology. The chapter in *Psychology in Social Context* in particular forms a bridge between his interest prior to his first publications in the history of psychology and subsequent publications. The present chapter will address the work done prior to his switch to history and theory, mostly in South Africa before 1965.

The key point here is that much of his South African work reflects a strong background in the sociology of knowledge, in which the figure of Karl Mannheim has loomed large. It will be argued that there are a number of continuities between these early publications and his historical/theoretical work. I will attempt to show that Danziger was steeped in this tradition long before he turned to history and theory of psychology. Indeed, one conclusion will be that his approach is consistently "sociological", and that the early work on empirical aspects of the sociology of knowledge informed his later work on the history and theory of psychology.

SOCIOLOGY OF KNOWLEDGE

What is the nature of the link between the kinds of knowledge produced and the social conditions under which it is produced? How are such relationships investigated? These are questions about the social roots of intellectual structures, which typically resort under the sociology of knowledge. Karl Mannheim has been a central figure in the study of the relationship between ideas and the structure of society. He defined one of the foremost problems of the sociology of knowledge as

> how and in what form did all the ways of thinking, currents of thought, meanings of concepts, and categories of thought come about that constitute the present state of our knowledge and the totality of our world views? (1986, p. 48–9)

In response to the epistemological question mentioned above, he arrived at the concept of "style" to group together ideas in terms of their form and content (Nelson, 1992). Ideational trends can be regarded as styles of thought, and he proposed that the analysis of styles of thought formed the basis of the sociology of knowledge. The empirical task for the sociology of knowledge was

> to reconstruct its historical and social roots; to explore the change of forms in this style of thought in relation to the social fates of the bearing groups. (Mannheim, 1986, p. 189, emphasis in original)

These styles are borne by specific social groups in response to their experiential conditions, influenced by that group's standing in wider society at a particular time in history. This is a formulation of social context as something socio-historical.

Mannheim's book on conservative thought was supposed to work out what an empirical sociology would look like. Nevertheless, his approach to the sociology of knowledge did not deliver fully on its empirical promise. After a reconstruction of Mannheim's research program, Nelson (1992) concludes that such a program could be realized, and that Danziger's work (1963b) in this tradition points to the way forward.

DANZIGER'S EMPIRICAL INVESTIGATIONS
OF STYLES OF THOUGHT

How does one study long-term psychological changes that are important in a historical context? How does one investigate empirically how macro-social factors and the development of knowledge are related? These are the methodological questions Danziger posed in the 1950s and 1960s, when he turned to Mannheim's sociology of knowledge (e.g. 1936) as a source of inspiration.

In one sense, South Africa presented an ideal "context" to investigate such questions. Social relations in the country were troubled and insecure. In *Ideology and Utopia* Mannheim analyzed a not too dissimilar state of affairs in the Weimar Republic, about an intellectual crisis situation within the context of a social and political crisis in the latter stages of the Republic. According to Nelson, Mannheim argued that

> in situations of group conflict the underlying worldviews, or more exactly the funda-mental designs, of the groups involved will form the cognitive basis for the articulation of styles of thought that explicitly defend the reactive or proactive lifestyle 'com-mitments' of the groups. Large-scale economic changes which displace the mode of living of social groups stimulate the production of styles of thought as groups realize that their existing ways of life are threatened. (1992, p. 36)

In all the studies discussed below, Danziger used existing socially-defined "race" groups in South Africa to produce the material for analysis. The reasons for this he gave himself (Danziger, 1963b). Firstly, there are historically specific factors that made race important in South Africa. Secondly, the social distribution of privileges occurs along racial lines, and is maintained by making race the principal administrative concept. Thirdly, race extends to all aspects of life; in fact, it is the foundation concept of the social and political order in South Africa. It was a society where no compromises were made about its racial structure, and where economic, political and social positions were rigidly defined. This made it relatively easy to detect and describe different styles of thought. Following Mannheim then, different

groups in South Africa ought to hold different social theories, and the question becomes an empirical one: how to detect them in different groups.

Three studies led up to Danziger's "Ideology and Utopia" paper (1963a). In the first study (1958a), Danziger started to explore the association between the social position of a group and its view of social structure and social causation. He asked two groups of students, whites and blacks, to write an autobiographical essay, imagining themselves in 50 years time. Thus it was an autobiography projected into the future, to allow them greater opportunity to discuss their lives in a wider social setting, and to obtain information about their life goals and aspirations. By asking participants to focus on the future rather than the present or the past, the instructions managed to avoid any argument over which view was "objectively correct"—a problem for the sociology of knowledge throughout its history. Earlier Allport and Gillespie (1955) also asked students to write about their plans, hopes and aspirations for the future, and this work followed that practice. In a later paper (1963c) Danziger thanked Allport and Gillespie for making available their sample of South African autobiographies.

Danziger however also was interested in individual processes, such as how manifestations of group differences entered into the personality of individuals. If they did, it ought to be possible to show empirically that individuals from different social groups differed in the values they held and the goals they set for themselves. To explore personal values, respondents were asked to respond to questions such as: "For what end would you be willing to make the greatest sacrifice of personal comfort, time, and money? (1958a, p. 318)." One of the consistent differences between the white and black (black African and Indian) students was that white students were concerned with private goals and aspirations, while black students mentioned benefits to their communities much more frequently, and had aspirations to serve that community. Allport and Gillespie (1955) similarly found a greater degree of what they called "privatism" among Americans, white South Africans, and New Zealanders, than among Egyptians, black South Africans, and Mexicans.

In a follow-up part of the study, these main results were given to the students a few months later and they were asked to account for them. The groups also differed in terms of the explanations they gave for this finding. Whites tended to explain the differences that emerged in terms that downplayed the existence of conflict between groups: they ascribed the differences mainly to factors related to group inferiority, and group traditions. Blacks gave more conflict type explanations for these differences, such as political and economic discrimination, and barriers to individual achievement. Thus it seemed as if the groups adhered to two types of social causation, tied to their position in society.

These findings provided support for some of the basic premises of the sociology of knowledge, Danziger argued. Whites, as beneficiaries of the social arrangement, were more conservative in their outlook, while blacks stressed the factor of social conflict, with the implication that things might change. Mannheim

defined ideology as "those complexes of ideas which direct activity toward the maintenance of the prevailing order" and utopia as "those complexes of ideas which tend to generate activities toward changes of the prevailing order (Wirth, 1936, p. xxiii). The white group's dislike of social change led them to deny the element of social conflict with its possibilities of social change (ideology), and the black group stressed conflict, with the resulting possibilities of change (utopia). Indeed, one might say that a difference of implicit social theory has been detected, in terms of how people conceive the structure of society and the relationships between groups.

In the second paper (1958b), group differences in the definition of the social situation were examined. In South Africa, this meant examining the evaluation by whites and blacks of the dominant pattern of their society, captured by the term "white civilization". White and black students were presented with a list of 14 features which "different people have claimed to be highly characteristic of white civilization in South Africa" (Danziger, 1958b, p. 340). They were asked to indicate which of these features they considered to be really characteristic of white civilization and which not. In addition, they were asked to respond to the same questions identified in the previous paper, and to complete an abbreviated version of Adorno's F scale.

Once again, differences in "styles of thought" could indeed be demonstrated between privileged and non-privileged groups in South Africa. Whites, as the beneficiaries of the social order (i.e. "white civilization"), overall tended to evaluate it more favorably than blacks, whom the system reduced to second class citizens. It showed also why South Africa was such a good example to study, because of the domination of a white minority over power. In a homogeneous society members shared a much more common definition of their social situation: "their position in the world, their goals and how to achieve them; they have a similar evaluation of their society as a whole and of their position in it" (Danziger, 1958b, p. 339). In a society split by conflict, opposing groups could be expected to define the social situation very differently.

The existence of styles of thought did not rule out the possibility that sub-systems existed within groups as well. Danziger examined differences within the white group, and found that the proportion of favorable valuations was much less among university students than among technical college students. The technical college students, Danziger speculated, might be more representative of the population as a whole, while the university students came under the influence of a more critical attitude at university. Differences also occurred within the black group: the proportion of unfavorable evaluations was slightly greater among African than Indian respondents. Africans had even fewer civil rights in South Africa than Indian respondents, and this difference in social position could explain this result.

Furthermore, the groups differed in the nature of the favorable items they chose to characterize "white civilization". Whites chose items such as "high

standards of morality in the sphere of family life", and "respect for law and order". This indicates that they perceived the social order as moral and just, as "white civilization" could claim some moral advantage. Blacks were only prepared to concede that it delivered material advantages to whites, by choosing items like "a superior system for the production of material goods". They rejected its claims to moral excellence; in fact, they rated it as immoral and unjust, by choosing items like "unjust oppression of nonwhite people". Phrased in more psychological terms, one could say that this is a difference in attitude, but "attitude" is conceived in a much more holistic and social fashion in this study than was the case in the more typical attitude surveys of the time.

Answers to the questions about personal values confirmed the previous finding that whites are more "privatistic" and blacks more "communal". How to understand this link? Danziger suggested that a group's orientation was determined by "certain positive pressures towards redressing real and perceived limitations on the group by means of group action." In less privileged groups, who were discriminated against, members "tend to internalize the social aspirations of the group so as to turn them into individual aspirations for each member" (Danziger, 1958b, p. 343). This convergence of social and individual goals occurred when the social system limited or blocked individual aspirations, simply because of the group they belonged to. For dominant groups, on the other hand, a conflict between public duty and individual interests emerged. For example, none of the white respondents mentioned a change in the social order as one of their personal desires. Some of them recognized the injustice of this order, so for these respondents there was a discrepancy between the definition of the social order and their personal aspirations. Whites resolved this by agreeing with statements about "abstract helpfulness", such as "reducing human unhappiness". The commitment therefore remained abstract and imprecise, which was quite convenient, because it was unlikely to lead to action. The more specific the social aim, the more likely it would lead to social action. In line with this, the black respondents mentioned aspects of specific helpfulness much more frequently, e.g. "establish a clinic in an African area".

> As long as the aim remains abstract and formal, its function may not really be that of re-orientating the individual towards social action, but rather that of assuaging the guilt that arises from the conflict between social ideals and private interest. For the socially oriented person, on the other hand, social aims naturally assume a concrete content, as they arise directly out of the demands of a specific external situation that have become identified with his individual interest. (ibid.)

Those white participants who gave the most favorable responses to "white civilization" tended to get higher scores on the F scale, as one could predict. Their acceptance of social discrimination and approval of the existing social situation were linked with authoritarian values and fascism as estimated by the F-scale. In the black group, authoritarian values were frequently associated with a critical

attitude to the existing social order, which Danziger argued had to do with the need for group solidarity. Thus one had authoritarian values espoused by both white and black groups, but for totally different reasons. To explain this, one had to go beyond the narrow confines of psychology again: "The interpretation of the pattern of 'authoritarianism' must always take into account the wider social context" (Danziger, 1958b, p. 345).

In the third paper, Danziger (1963a) used the future autobiographies as a method of assessing another aspect of the inter-relationship between macro-social factors and ideas. "Economic growth", and the differences in growth patterns between countries, were not areas in which social psychologists showed much of an interest. Apart from McClelland's work on achievement motivation, psychologists had little to say about the requirements of economic growth, particularly in "under-developed" countries.

The question then becomes how to investigate psychological factors that are associated with sociological factors involved in economic growth. The future autobiographies were seen as a promising technique to measure the presence of "action tendencies" (Danziger, 1963a, p. 17) in individuals, which could be linked to certain sociological factors, such as participation in modern economic and administrative processes. The action tendency in this study turned out to be the tendency toward self-rationalization.

Max Weber (1947) identified one of the core components of modernization in terms of a growing process of rationalization of various spheres of society. It is characterized by elements such as specialized institutions, the adoption of bureaucratic standards, the separation of private and public, and secularization. Danziger used the term rationalization to indicate the organization of "actions into a system which constitutes the optimum arrangement of means for bringing about a certain end" (Danziger, 1963a, p. 17). In such a system custom was no longer blindly accepted as a justification for organizing society, and was gradually extended, as the economy in these countries became more industrialized and administration more bureaucratized.

As larger areas of social life are rationalized, individuals become "rationalized" as well. Mannheim (1940) recognized this, and called the change in the individual's own attitude to his/her life "self-rationalization". Life has to be seen as a long-term enterprise, in which each step has to be planned and calculated in terms of how it will contribute to achieving ultimate goals. The criterion for the rationality of the actions of individuals in this context was how it contributed to career success. It involved the "calculating control of impulse in the interests of a deliberately formulated life-plan" (Danziger, 1971, p. 292). For Danziger, this implied a rigorous control of impulse, and the application of a strict, objective time scheme to one's life. It stands to reason that individuals would differ in the degree to which they manifested these tendencies, and it should therefore be possible to measure these individual differences. Self-rationalization is associated with larger

social processes through a group's involvement in rationalized economic and administrative processes. Where members of a group have been exposed to such processes over a long period of time, higher levels of self-rationalization should be present when compared to groups where this exposure has been recent and incomplete, argued Danziger.

The instructions for the autobiographies were slightly different from before. Students were asked to begin at the present, and to write a few paragraphs concerning their expectations, plans and aspirations for the future. From these essays, an index of self-rationalization was calculated from 7 variables, such as: ego-reality statements (realistic statements about the writer's personal future); non-career values (the writer's commitment to values that conflict with the pursuit of pure self-interest); objective time reference (rationing of time for its most efficient use); and time structure (the number of distinct stages on the life path). The presence of these seven variables in the biographies was scored and weighted, resulting in a scale on which 25 was the highest possible score and 0 the lowest. Individuals who were high in self-rationalization would exhibit

> a very realistic level of planning, a relative absence of unrealistic fantasy and of non-career goals, a concentration on personal rather than community goals, a pre-occupation with economic incentives, and the use of a well-articulated temporal structure shown by precise time references and orderly succession of life stages. (Danziger, 1971, p. 292)

The hypothesis that participation in rationalized economic and administrative processes will be substantially related to self-rationalization was supported. First, African males manifested a far lower level of self-rationalization than English-speaking white males because, Danziger argued, of their incomplete involvement in rationalized social institutions and the special limitations imposed upon them by an irrational system of social domination. When compared to Allport and Gillespie's (1955) data, these differences between black and white South Africans ran parallel to the differences between respondents from highly developed and the "underdeveloped" countries these authors used. Furthermore, Allport and Gillespie showed Afrikaans-speaking students to be significantly below English-speaking students on the mean index of self-rationalization, reflecting their differences in degree of involvement in the modernizing sectors of the economy. By the time of Danziger's study, however, this difference was no longer significant, in line with Afrikaans speakers' increasing participation in the modernizing economy.

Thus the future autobiography seemed to provide a technique for objectively assessing a pattern of rationalization in large groups of respondents. Once such a technique was available, it became possible to investigate the psychological aspects of the pattern of self-rationalization. In this paper the economy was brought into reciprocal influence relation with the psychology of the individual.

The key paper in this series was published in 1963(b). The title, "Ideology and Utopia in South Africa" was a deliberate reference to Mannheim: "I called the paper in the British journal 'Ideology and Utopia in South Africa' which is a direct take on Mannheim's book" (Danziger, in Brock, 1995, p. 13). He asked (mostly) university students (84 African, 51 Indian, 53 Afrikaans-speaking white, and 251 English-speaking white) to write essays projecting future social changes in South Africa (Danziger, 1963b, pp. 65–66).

From these "future histories" he analyzed the styles of thought of the different social groups. For the analysis of the future autobiographies collected in this study, he devised a five-fold typology of styles of thought, or dominant type of historical orientation: Conservative; Technicist; Catastrophic; Liberal; and Revolutionary. The assignment of student writing content to one of these styles was determined by the presence of four characteristics in their essays: (a) the attitude to and interrelationship of the present and the future; (b) interrelationship of historical means and ends; (c) the conception of social change; and (d) the conception of social causality. The essay was assigned to one of the five types in terms of which one occurred most frequently in terms of the four criteria.

The Afrikaans-speaking white students mostly exhibited Conservative and Technicist orientations to the future, while English-speaking whites were mostly Catastrophic and Conservative in their orientation. Indian students were Liberal and Revolutionary, while African students were Revolutionary and Liberal. Thus "the frequency of the various types of historical orientation conforms broadly to the position of the different groups in the social structure" (1963b, p. 70). The Afrikaans-speaking group was at the head of the power hierarchy and had the highest frequency of conservative types, while the African group, which was lowest in the hierarchy, produced the highest frequency of revolutionary types.

As in the 1958(b) paper, findings clearly showed that differences existed within groups as well. Afrikaans-speaking white students, for example, who tended to adopt either conservative or technicist historical orientations, included some catastrophic or liberal orientations. In addition, the extent of this range varies for different groups at different times. In societies that were undergoing rapid social change, Danziger believed future autobiographies provided a valuable technique for establishing "the crucial links between changes in social structure and changes in personality structure" (p. 27).

These studies showed clearly that it was possible to detect differences in contemporary thought styles, especially in highly stratified, unstable societies, using empirical methods as described. Danziger came to the conclusion that the range of available thought was socially determined, and that social position determined the range of available historical orientations for the members of each group. Furthermore, the situationally transcendent ideas that were identified could be regarded as attempts at subjectively mastering the basic tensions in society.

In 1963 samples from future biographies collected in 1952, 1956 and 1962 from a total of 162 African high school students were analyzed. Danziger (1963c) had no less a target than a "historical psychology" in his sights; a psychology concerned with "that deeper surge of change represented by the reconstruction of values and perspectives in the context of complex historical developments" (p. 31). The application of quantitative methods of content analysis in this regard was very different from the conventional employment of these methods.

At the time that the first autobiographies were collected, apartheid still had some degree of flexibility, though it became more and more coercive and uncompromising as the years progressed. By 1962, the last time that the autobiographies were collected, the lives of black Africans were under the complete control of the apartheid system. In 1961 political activity in the black community went underground, and acts of sabotage began toward the end of 1961. This led to more repressive measures from the apartheid state. The empirical question in this publication was: How would these changes in imposed social control and repression affect the psychological future of African high schoolers?

The results again provided support for an interpretation sympathetic to the sociology of knowledge. For a start, a massive majority of essays expressed complete opposition to government policies, with not a single statement of identification with the system. Forty-six percent predicted a violent overthrow of the regime. These percentages did not change from 1950 to 1962. There was also a consistent increase in a preoccupation with socio-political problems, and a tendency to see the future in social rather than individual terms. It is not too difficult to see these developments as reactions to changing conditions of political repression. The content of the psychological future as reflected in the future autobiographies also changed as a result of these structural changes. Both the goals of economic success and community service declined over time, to be replaced by political activity goals, expressed in the cause of African nationalism. "The intensification of authoritarian political control is having the effect on the individual educated African of defining his future in political terms" (Danziger, 1963c, p. 39).

These empirical studies were conducted during one of South Africa's most politically repressive periods. The National Party had started to implement its apartheid policies vigorously and systematically since its election into power in 1948, which led to large-scale confrontations with black resistance organizations in the 1950s and 1960s. Thus this period of extreme social instability in the country was an almost ideal-typical setting to examine Mannheim's theories regarding the role of situationally transcendent ideas about the future of society. Apartheid ideology and practice structured racial and political consciousness of different groups to such an extent that they failed to develop a shared style of thought. For the most part whites saw the situation as "normal" and generally acceptable, while blacks saw it as ripe for radical change. Danziger's empirically based historical psychology reconstructed the social and historical roots

of these ideologies and utopias in terms of the positions of the groups holding them.

The discussion of these studies identified and emphasized the sociological influences in Danziger's work. But what about psychological influences? The social psychology of Kurt Lewin certainly deserves some mention in this regard. One clue to its influence on the early work of Danziger is provided by the prominence given in his empirical papers to "the psychological future" as experienced by respondents. Danziger hypothesized that one of his findings, the decline of the use of a temporal framework by his black respondents, could be explained in terms of special limitations placed on them from 1950 to 1962. He ascribed to Lewin (1954) the hypothesis that a decline in the "differentiation of the psychological future may well be the result of externally imposed frustration" (Danziger, 1963c, p. 37). In Lewin's work, the psychological meaning of actions was emphasized, which was derived from the larger structure within which such actions were embedded. For example, the state of the person and that of his/her environment were not independent of each other—the person lived in a psychological environment (Lewin, 1954, p. 918). For Lewin the behavior of a person always was part of the larger situation, and thus the object of investigation in psychology had to be the "person-in-a-situation". The psychological meaning of an action therefore was not fixed, but depended on the context within which it occurred. For example, in Lewin's studies in the 1930s at Iowa on "group climates" (Lewin, Lippitt & White, 1939), major differences emerged between the boys in the "authoritarian", "democratic", and "laissez-faire" conditions. In other words, differences in their behavior depended on differences in the social conditions in which they found themselves.

Additional resemblances between Lewin's and Danziger's work are the tendency to confront significant social issues in their research, and the acknowledgement that human actions take place in a temporal domain as well, rather than being a characteristic of a static "personality".

Lewin's work formed a bridge to Gestalt psychology for Danziger. His work on group climates point to Lewin's preference to work with holistic units in a non-elementaristic fashion. Individuals were not studied in isolation, but as participants in whole situations. As Danziger wrote in *Constructing the Subject* about Lewin, "types of psychological context" (p. 177) rather than individuals become the real objects of psychological investigation. Another linkage to Gestalt theory is through Solomon Asch's attempt to develop a psychology of social life through using Gestalt theory. For Asch, one level of human motivation was that human beings "crave society" (1952, p. 324)—they have a "social interest". Behaviorist and psychoanalytic theories of social interest firstly

> find no place for precisely the phenomenon with which enquiry should begin—the presence of a direct overflowing interest in other human beings, in the life of groups, and in the need to participate actively in them. (p. 332)

From this brief discussion of psychological influences in the early empirical work in the sociology of knowledge tradition, one can say that they were European rather than Anglo-American in origin, despite the fact that Danziger received his formal training in the latter.

FURTHER EMPIRICAL STUDIES

In the 1980s and '90s a number of studies revisited Danziger's empirical sociology of knowledge approach, to study psychological concomitants of political change in South Africa. Du Preez, Bhana, Broekmann, Louw and Nel (1981), Louw (1983) and Du Preez and Collins (1985) provided time series data on social orientation. They established that Afrikaans-speaking whites had changed most over time, from a conservative position (nothing will change politically) to a liberal position (gradual, controlled change will take place). African and Indian groups changed the least in future orientation. In addition, there was no dominant or transcendent historical perspective that could unite all groups. Whites predominantly saw the future as catastrophic, while black groups were more optimistic. These studies were conducted at a time when the country again was in turmoil, as a result of the apartheid state's military response to black resistance, and in the mid-1980s several states of emergency were declared to quell popular uprisings.

In February 1990 Nelson Mandela was released from prison into a very polarized society. As the negotiations for a new political dispensation started, levels of violence actually increased, and were particularly high between 1992 and 1994. The political solution reached at these negotiations during the first part of the 1990s culminated in 1994 in the first democratic elections in South Africa. During this time, Finchilescu and Dawes (1999) asked adolescents, both prior to and after the foundation of democracy in South Africa, to write an essay in which they predicted the future of South Africa in the next decade. Only two future scenarios appeared in the essays: Catastrophic and Liberal. The Revolutionary outcome virtually disappeared from the essays written by black African youth, and they now expected Liberal futures—society would be peacefully transformed under government guidance. The orientations produced by coloured and Indian adolescents shifted from 1980 to 1996 to be more similar to whites than they were to Black Africans. These groups produced high percentages of essays with a Catastrophic orientation to the future: the future held chaos, violence, and social upheaval. Thus the authors established again that wide differences in the perceptions of the youths from the various population groups existed. Finally, there also were a large number of essays without clear future themes, many more than in previous studies. They ascribed the latter finding to the lack of a clearly defined structural conflict, and a state of confusion about the future.

In these studies we again recognize the important elements of the historical-psychological approach Danziger had in mind earlier. The differences in the perspectives between social groups, and the changes they represented, must be studied and understood "in the context of complex historical developments" (Danziger, 1963c, p. 31).

IMPLICATIONS

Danziger's early empirical studies in the sociology of knowledge contain at least three continuities with his later work in the history and theory of psychology. These are concerns with history, context, and method.

The first continuity refers to the recognition of history. Indeed, the strength of the sociology of knowledge, in Mannheim's tradition, is its recognition of the importance of socially transcendent ideas—ideas that point to the past or the future. Danziger similarly concerned himself with the subjects' temporal orientation—past, present, and future. He argued that the temporal dimension was in fact the dominant stylistic dimension, as events are ordered on a time scale (Danziger, 1963b). For example, a revolutionary style of thought emphasizes present tensions, which will be removed in future by social disruptions at one or more strategic moments. Thus a concern about the future of society, as indicated by future biographies or future histories, introduces temporal orientation as a dimension into empirical research.

Context assumed two meanings in these publications. In one sense, Danziger worked in a specific political context himself, which allowed him research possibilities not so available elsewhere. Two prominent aspects relating to the South African setting of this work can be identified. First, disciplinary boundaries were much less rigid than they were in American social science at the time. In an interview (Brock, 1995, p. 11) he said, referring to South Africa,

> That was the other thing that began to strike me at that time: the tremendous hold that disciplinary loyalties had on social psychologists in North America when compared to their counterparts in some other parts of the world. For us it really wasn't that important whether a person was a psychologist or a sociologist or an anthropologist.

This made it easier to follow research avenues suggested by the sociology of knowledge when one looked for explanations of human actions.

A second contextual factor linked South Africa to the recognition of history in this work. In the "underdeveloped" (as they were still called) countries of the world social relations often were unstable enough to cast doubt on how they could be maintained in future. Where the future of society was in doubt, situationally transcendent ideas flourished like in the Europe of old, Danziger argued. South Africa of the 1950s and 1960s was a country where doubt about the future of society

was intense, because few could see a way out of the conflicts created by the race-based policies of the government at the time. Rigid social distinctions based on race dominated all aspects of life, so much so that situationally transcendent ideas developed in the society could be expected to be virtually mutually exclusive, depending on the positions of the contending groups. This is a classic situation for the sociology of knowledge.

Context also was used in a sense much closer to how it would be used later in historical-theoretical work. The 1958(a) paper recognized explicitly the possibility that social context may play a more important role in psychology than generally accepted. Black South Africans expressed a stronger desire for social equality and social freedom than for the satisfaction of immediate private needs, and this reflects on psychological theories of human motivation. This is more in agreement with Asch, says Danziger, and less in agreement with some of the traditional biologistic theories of motivation. In his *Social Psychology*, Asch (1952) identified the "biological doctrine" as one of the explanations of the social nature of human beings. This explanation entered psychology under the aegis of behaviorism, argued Asch, and as a result, human social actions were learned because "they bring the individual directly or indirectly the gratification of primary needs" (p. 13). For Danziger, however, there is another implication here: "one can only raise the question of the extent to which even supposedly scientific theories in psychology are affected by the social context in which they arise and flourish" (1958a, p. 323). Also, in explaining the pattern of authoritarianism exhibited by his respondents, the wider social context had to be taken into account (Danziger, 1958b, p. 345). Such a contextualist position is of course part and parcel of the sociology of knowledge, in which concepts have a basis in specifiable contexts.

Methodologically, two aspects of these studies deserve mention. Danziger was searching for empirical methods in social psychology that were responsive to the factors of history and context. A major concern was that the methods used by social psychologists, in attitude surveys for example were too reductionistic. Attitude surveys normally start off with a collection of separate elements in order to arrive at a measure of the whole. In addition, they place respondents in the role of passive selectors of pre-structured categories. In *Constructing the Subject*, he pointed out that the standard laboratory experiment, with its emphasis on isolating individual "stimuli", also was reductionistic in its approach. The value and attractiveness of Mannheim's approach lay in its reversal of this practice: it was concerned with social totality, and with its active construction by social agents. Ideological or utopian attitudes were treated as wholes, since they arose when the future of society as a whole was in doubt. The meaning that social events had for the individual was determined partly by the kind of ordering used by the social groups s/he belonged to.

South Africa again provided fertile ground to show that a tendency toward an individualist orientation and away from a socially oriented interpretation will lead

to meager insights. The psychological aspects of personal lives in countries like South Africa often were of a secondary nature. There were larger scale, macro-sociological factors that had to be considered first. Also, to look for the starting point of social change at the level of individual motivation was simply a mistake. For example, in terms of factors retarding economic growth, he argued that

> As far as South Africa is concerned, one cannot dismiss the possibility that the forcible stifling of political aspirations is indirectly responsible for the low level of discipline, morale and enthusiasm of many African workers. (Danziger, 1963d, p. 397)

Thus an understanding of "the problem of African workers' productivity" cannot first be sought at an individual level. By the same token, however, sociological factors were not the only ones operating here. They interfaced in a complex pattern with individual characteristics of persons. Take for example the operation of laws that barred black people from advancing beyond the lowest level jobs:

> ... if no amount of personal achievement will lift the individual beyond the social status of a second-rate creature who is not capable of determining his own future, then we should not be surprised if interest in achievement remains at a low level. Large-scale individual efficiency and the maintenance of a system of social stratification based on inborn characteristics like skin color would seem to be largely incompatible. (p. 398)

The challenge of conceptualizing the relationship between the individual and social interpretations of course remained with social psychology up to the present (see the discussion on levels of explanation below).

In the chapter in Buss (1979), Danziger merges the three elements of history, context and method. History now takes central stage, for the first time in his publication record. The methodological focal point now shifts away from empirical methods in social psychology to historiography: how to practice the history of psychology. In this practice, the influence of the sociology of knowledge is still clearly discernible. I have already indicated that Buss placed the text squarely within the sociology of psychological knowledge, in particular in the debate between "internal" and "external" historians of psychology. Danziger approaches this debate by analyzing the institutionalization of American and German psychology. The rise of the discipline of psychology, Danziger argued, depended on the invention of a role that did not exist before, that of the professional practitioner of the new science. This new role depended on the society in which such roles were established, with the result that what was defined as "psychology" differed quite substantially between the USA and Germany. The reasons for this were clearly not just internal to the discipline. German and American psychologists had to take into account the norms and interests of existing power groups in their quest to institutionalize psychology, but the power groups psychologists had to address were very different in the two countries. In Germany, it was an academic and professional establishment

dominated by philosophy. In the USA, however, universities and the resources they controlled were much more allied to the business sector, or to politics. The difference in social context determined the different forms that psychology took in quite fundamental ways in the USA and in Germany.

In this connection Danziger evoked the concept of legitimation. This terminology too has its background in a publication on South Africa, when he published a paper (Danziger, 1971) in which analyzed strategies of legitimation of social power, using the successive legitimations of apartheid as a case in point. He now draws from a slightly different tradition in the sociology of knowledge, that established by Max Weber.

INTELLECTUAL INTEREST

In the examination of legitimation strategies that led to differences in the institutionalization of psychology in Germany and the USA, Danziger tried to overcome the dualism created between internal and external factors in the development of the discipline. To accomplish this, he introduced an important historiographical device that would link these two opposing poles, in the concept of intellectual interest. Intellectual interest mediates between external and internal forces operating on the development of a discipline, he argued. It faces both inward and outward:

> outward, in that it serves to legitimate the activities of its practitioners vis-à-vis significant target groups; inward, in that it establishes the norms by which the work of practitioners is judged. (Danziger, 1979a, p. 38)

In Germany, psychologists had to convince an academic and professional establishment dominated by philosophy of the acceptability of the knowledge claims of the new discipline. In the USA, however, if psychology was to emerge as a recognized, independent discipline, it had to present itself as acceptable to business or political power groups. Thus psychologists presented themselves as the scientists of behavior, and ultimately had as their goal the "prediction and control of behavior".

Intellectual interest therefore is the instrument of legitimation, both "internally" and "externally". Internally, it holds together the practitioners of a field around the subject matter, goals and methods of the discipline. Outside the discipline, it represents an attempt to convince powerful groups of the acceptability of the discipline's work, because there is a compatibility of intellectual interests between the new discipline and these powerful groups. The concept of intellectual interest thus makes it possible to overcome the absolute separation of "social factors" and "intellectual content", that was so troublesome in a positivist sociology of science (e.g. Ben-David & Collins, 1966). Indeed, Danziger (1979a) turns to

"intellectual interest" as a device to understand "context" after strongly criticizing the positivist approach of these two authors.

In the 1980s Doise (1986) called this a problem of levels of explanation or analysis. Doise argued that by framing the relationship between the individual and the social in terms of a dualism, one faces the charge of reductionism at either extreme. He argued for four levels:

- Intra-individual levels of analysis are normally characterized as "psychological" explanations, such as the authoritarian personality.
- Inter-individual or situational levels of analysis involve processes between individuals, such as social comparison theory.
- Positional levels of analysis regard differences in position or social status, normally based on factors such as gender, race or class, to account for findings of a study
- Ideological levels of analysis emphasize the general conceptions of social relations that serve to legitimize the existing social order.

Those who criticized mainstream social psychology in the 1980s, at least as it was practiced in the USA, stated that it typically focused on the first two levels of analysis.

The sociology of knowledge approach chosen by Danziger for the empirical studies in social psychology made it possible to include all four levels of analysis in the explanation of his findings. But the dualistic framing of a choice between internal and external developments in psychology in the historiography of psychology also implied a level of analysis problem. What Danziger did by introducing the notion of intellectual interest was to reunite the internal and the external; to show that the problem arises when it is formulated in terms of a choice to be made. The tendency for psychology to give preference to individualistic levels of analysis has been discussed earlier. The sociology of knowledge, on the other hand, privileges macro-social structures, and social relationships within those structures. Sociologists of knowledge generally imply that in the relationship between knowledge and society, "the social" has primacy. In his chapter in Buss (1979), Danziger tried for the first time to overcome this dualism in regard to the history of psychology by showing how the intellectual interests of the community of specialist psychologists will mediate the relationship between psychological knowledge and interests and structures in the wider society.

CONCLUSION

In later years, this way of surmounting implied dualisms via mediating devices, became quite a familiar way of working for Danziger. In *Constructing the*

Subject (1990), and other publications (e.g. Danziger, 1993), he used the notion of investigative practices to perform a similar historiographical function to that of intellectual interest. Earlier he spoke of different patterns of investigative practice, such as the Leipzig and Paris models, and "American innovations" (Danziger, 1985). Investigative practice has a logical dimension in guiding the research work of psychologists, but it also has a social dimension. For example:

> the individual investigator acts within a framework determined by the potential consumers of the products of his or her research and by the traditions of acceptable practice prevailing in the field. Moreover, the goals and knowledge interests that guide this practice depend on the social context within which investigators work. (1990, p. 4)

The social context includes

> the pattern of social relations among investigators and their subjects, the norms of appropriate practice in the relevant research community, the kinds of knowledge interests that prevail at different times and places, and the relations of the research community with the broader social context that sustains it. (p. 5)

This typical way of working can be discerned in *Naming the Mind* (Danziger, 1997) as well, where the growth of attitude research is ascribed to two main factors. The first of these came from outside the discipline in the form of public interest, while the second factor was internal to the discipline and involved finding a way to measure attitudes. Thus investigative practice becomes the primary medium through which social forces have shaped the discipline.

Although investigative practices are claimed as the media through which social interests have been reflected, the analysis in later years went a little further, to include the construction of psychological objects themselves, and how investigative practices constituted such objects (Danziger, 1993). The embeddedness of psychology in extra-disciplinary contexts has implications for the very objects of psychological study. For example, with regard to personality and its assessment,

> Their construction of 'personality' or 'character' as an object of knowledge was strictly confined by the rather severe limitations of the social context in which their investigations originated. (Danziger, 1990, p. 171)

To be perceived as legitimate, psychology and the objects of its study could not stray too far from the local cultural definitions of their task. And here we are back to the ideological component of psychological knowledge, that different aspects of psychology will be sanctioned by different societies, and that psychology will build its cultural values into its procedures. For Danziger, the world of psychology

is a constructed world, and historians of psychology must study the constructive activities that produced it.

REFERENCES

Allport, G.W. & Gillespie, J.M. (1955). *Youth's outlook on the future*. New York: Doubleday.

Asch, S.E. (1952). *Social psychology*. Englewood-Cliffs, NJ: Prentice-Hall.

Ben-David, J. & Collins, R. (1966). Social factors in the origin of a new science: The case of psychology. *American Sociological Review, 31*, 451–465.

Brock, A. (1995). An interview with Kurt Danziger. *History and Philosophy of Psychology Bulletin, 7*(2), 10–22.

Buss, A.R. (Ed.) (1979). *Psychology in social context*. New York: Irvington.

Danziger, K. (1958a). Self-interpretations of group differences in values. *Journal of Social Psychology, 47*, 317–325.

Danziger, K. (1958b). Value differences among South African students. *Journal of Abnormal & Social Psychology, 57*, 339–346.

Danziger, K. (1963a). Validation of a measure of self-rationalization. *Journal of Social Psychology, 59*, 17–28.

Danziger, K. (1963b). Ideology and utopia in South Africa: A methodological contribution to the sociology of knowledge. *British Journal of Sociology, 14*, 59–76.

Danziger, K. (1963c). The psychological future of an oppressed group. *Social Forces, 42*, 31–40.

Danziger, K. (1963d). Some social psychological aspects of economic growth. *South African Journal of Science, 59*, 394–398.

Danziger, K. (1971). Modernization and the legitimation of social power. In H. Adam (Ed.), *South Africa. Sociological perspectives* (pp. 283–300). London: Oxford University Press.

Danziger, K. (1979a). The social origins of modern psychology. In A.R. Buss (Ed.), *Psychology in social context* (pp. 27–45). New York: Irvington.

Danziger, K. (1979b). The positivist repudiation of Wundt. *Journal of the History of the Behavioral Sciences, 102*, 143–148.

Danziger, K. (1985). The origins of the psychological experiment. *American Psychologist, 40*, 133–140.

Danziger, K. (1990). *Constructing the subject*. Cambridge: Cambridge University Press.

Danziger, K. (1993). Psychological objects, practice, and history. *Annals of Theoretical Psychology, 8*, 15–47.

Danziger, K. (1997). *Naming the mind: How psychology found its language*. London: Sage Publications.

Doise, W. (1986). *Levels of explanation in social psychology*. Cambridge: Cambridge University Press.

Du Preez, P., Bhana, K., Broekmann, N., Louw, J. & Nel, E.M. (1981). Ideology and utopia revisited. *Social Dynamics, 7*, 52–55.

Du Preez, P. & Collins, P. (1985). Ideology and utopia in South Africa: Twenty years after. *South African Journal of Political Science, 12*, 66–78.

Finchilescu, G. & Dawes, A. (1999). Adolescents' future ideologies through four decades of South African history. *Social Dynamics, 25*(2), 98–118.

Lewin, K. (1954). Behavior and development as a function of the total situation. In L. Carmichael (Ed.), *Manual of child psychology* (pp. 919–970). New York: John Wiley.

Lewin, K., Lippitt, R., & White, R. (1939). Patterns of aggressive behavior in experimentally created "social climates". *Journal of Social Psychology, 10*, 271–299.

Louw, J. (1983). Changing expectations of the future. In J.B. Deregowski, S. Dziurawiec & R.C. Annis (Eds.), *Expiscations in cross-cultural psychology* (pp. 403–413). Lisse: Swets and Zeitlinger.

Mannheim, K. (1936). *Ideology and utopia*. London: Percy, Lund, Humphries & Company.

Mannheim, K. (1940). *Man and society*. London: Kegan Paul.

Mannheim, K. (1986). *Conservatism. A contribution to the sociology of knowledge*. London: Routledge & Kegan Paul.

Nelson, R.D. (1992). The analysis of styles of thought. *British Journal of Sociology, 43*, 25–54.

Weber, M. (1947). *The theory of social and economic organization*. London: Hodge.

Wirth, L. (1936). Preface. In K. Mannheim, *Ideology and utopia* (pp. xiii–xxxi). London: Percy, Lund, Humphries & Company.

CONTROLLING THE METALANGUAGE

AUTHORITY AND ACQUIESCENCE IN THE HISTORY OF METHOD

ANDREW S. WINSTON

In *Naming the Mind*, Danziger (1997) analyzed the emergence of the fundamental categories of psychological inquiry. Challenging the implicit assumption that behavior, personality, intelligence, learning, and motivation can be understood as natural kinds, he carefully explicated their origins and dynamics as negotiated products of scientific communities. In Chapter 9, he focused on the introduction of the concept of *variables* in psychological discourse and how this change profoundly altered the shape of psychological inquiry during the 20th century. Danziger employed the term "metalanguage" to describe the shared discursive practices regarding method that came to be nearly universal in mainstream psychology, and indeed helped to define the mainstream.

This aspect of Danziger's work has overlapped with my own. Starting in 1988, I began to ask how the terms *independent variable* and *dependent variable* became universal in introductory psychology textbooks, and how these terms were used to define a relationship between causality and experimentation. By the 1970s, all North American psychology students were taught that an experiment consists of manipulation of an independent variable while holding all other variables constant and observing the effect on a dependent variable, and that this is the best or sole method for the discovery of causes. My interest in the source of this methodological dictum lead to a number of investigations of textbook

conceptualizations of experimentation, ideas of "cause" in modern psychology, the influence of Ernst Mach's philosophy of science, and the place of experimentation in social psychology (Winston, 1988, 1990, 2001; Winston & Blais, 1996; MacMartin & Winston 2000). I argued that the definition of experiment and its relation to causality was, for the most part, "home-grown" in psychology rather than imported into the discipline, though related to Machian philosophy of science in important ways. Further, I argued that this conceptualization profoundly shaped the way psychological questions were asked and answered.

In this chapter, I have three aims. First, I will augment and amend aspects of my earlier work. Second, I will highlight some commonalities and differences of emphasis in Danziger's and my analysis of change in the metalanguage. In this regard, I am grateful for the discussions that he and I have had over a number of years. Third, I examine the process of change in metalanguage by considering a case in which such a change was blocked by powerful authorities, during the same period that the contemporary definition of experiment was introduced.

EARLY USE OF THE CONCEPT OF *INDEPENDENT VARIABLE*

Between 1932 and 1934, a number of leading psychologists began to employ a new way of speaking about the "causes of behavior." R. S. Woodworth, E. G. Boring, and E. C. Tolman, substituted the concept of the *independent variable* for the concept of *cause*, a term with a long and problematic history, accompanied by much metaphysical baggage. Each of these authors used this term in a slightly different way. The introduction of the concepts of *independent variable* and *dependent variable* into psychological discourse was an important event in terms of providing a common language for the discussion of investigations based on highly divergent theoretical systems. In this analysis, I was interested in how these terms came to define what an experiment was, and how this conceptualization then encouraged certain kinds of inquiry.

In my previous work, I suggested that the terms *independent variable* and *dependent variable* did not appear in psychology until the 1930s (e.g., Winston & Blais, 1996). A more recent search of the expanded PsycINFO database indicated that some papers of the 1920s may already have used these phrases. For example, "independent variables" appears in the abstracts of both Culler (1927) and Heidbreder (1927). However, their use is not the same as in the 1930s: the term is used to mean "variables acting independently of each other," rather than experimentally manipulated factors.[1] Danziger (1997) noted that William James (1890/1950) had used the term "independent variable" in the Principles (I, p. 59), but here again, the meaning in context appears to be "independent of each other," not the experimental factor which is manipulated by the experimenter.[2] These early

uses of "independent variable" are not clearly derived from the original mathematical meaning, described below. In this case, the use of the same term is misleading for the contemporary reader. But in other cases, a slightly different term is used to mean something close to the 1930s meaning of *independent variable.*

One example of the use of a related term is from William Stanley Jevons (1874). In his influential *Principles of Science*,[3] he described the nature of experiment in terms of active manipulation and identified the terms to be used:

> Almost every series of quantitative experiments is directed to obtain the relation between the different values of one quantity which is varied at will, and another quantity which is thereby caused to vary. We may conveniently distinguish these as respectively the *variable* and the *variant*. (p. 440)

Both the terms "variant"and "variate" were commonly used for variable at the turn of the 19th century and in the early 20th century. In his highly influential *Statistical Methods for Research Workers*, R. A. Fisher (1925) used the terms "independent variate" and "dependent variate," which he introduced to explain regression functions and their graphic representation. But he did not present this idea as a definition of experiment nor as the factor explicitly manipulated by the experimenter.

The original meaning of *independent variable* and *dependent variable* had nothing to do with experimentation. The concepts of function and variable were clearly present in the work of Leibniz, although his concept of a function was certainly not identical to the modern one. The specific terms *independent variable* and *dependent variable* were introduced by John Radford Young in the *Elements of the Differential Calculus* (1833): "on account of this dependence of the value of the function upon that of the variable, the former, that is y, is called the *dependent* variable, and the latter, x, the *independent* variable" (p. 2). Johann Gustave Lejeune Dirichlet provided the modern definition of a function in 1837, which O'Connor and Robinson (2002) translated as follows:

> If a variable y is so related to a variable x that whenever a numerical value is assigned to x, there is a rule according to which a unique value of y is determined, then y is said to be a function of the independent variable x.

The important feature here is that the independent and dependent variable are interchangeable: there is no implication that x is the cause of y, and the relationship can be expressed as y = f(x) or x = f(y). There is no requirement that the independent variable be manipulated, or that they have the asymmetrical status noted by Danziger (1987). As I have described elsewhere (Winston, 2001), it is in the writings of Ernst Mach that the connection between functions, experiment and explanation is outlined. By the 1880s, Mach was clear that the concept of a function, expressed mathematically, was to replace the metaphysically tainted concepts of cause and effect. Functions were purely descriptive and their economical

descriptive power was for Mach the only proper and useful form of scientific explanation. Once Mach and others began to speak of mathematical functions as the replacement for causal statements, it was natural to substitute the terms proper to functions, independent and dependent variables, for statements of cause and effect.

However, it is possible to overstate the role of Mach in introducing the more general talk of variables in Psychology. As Danziger (1997) noted, texts and articles on statistics already made the concept of variables familiar to psychologists. With the increasing introduction of statistics into research and teaching during the first three decades of the 20th century, variables, variates, or variants would figure prominently. The terms *independent variable* and *dependent variable* were perfectly appropriate for any discussion involving a regression line. The literature of Applied Psychology, especially discussions of prediction, used *independent variable* in its original sense from the mathematics of functions during and after the 1930s (e.g., Wilson & Hodges, 1932). The use of these terms for prediction of a criterion measure from a test eventually caused some friction between experimentalists and multivariate researchers as experimentalists came to use *independent variable* to refer only to manipulated conditions (see Winston, 1990).

INDEPENDENT AND *DEPENDENT VARIABLES* APPEAR IN PSYCHOLOGY TEXTBOOKS

In introductory textbooks, the earliest use of the terms *independent variable* and *dependent variable* to define experimentation was in Woodworth's (1934) *Psychology*. In the third edition of his widely used textbook, he altered and narrowed the definition of "experiment" to include only studies in which a variable was explicitly manipulated:

> An experiment is one means of obtaining observations bearing on a definite question . . . the experimenter "controls the conditions." He does not let things happen at random; in the ideal experiment he has all the factors under his control that have any influence on the process to be observed. The result shows him what happens under certain known conditions. He varies one factor and notes the difference that makes in the result. The rule for an ideal experiment is to control all the factors or conditions, to keep all of them constant except a single one—which is then the *independent variable*—and to vary this one systematically and observe the results. The results are changes in the *dependent variable*. When he finishes his series of experiments, he knows the changes in the *dependent variable* which are produced by changes in the *independent variable*. (1934, p. 18)

Although Woodworth was the first to present the now universal textbook definition, E. G. Boring (1933e) had defined experiment as the manipulation of an independent variable in the previous year, in his *Physical Dimensions of Consciousness*, a book Boring designed as a tribute to E. B. Titchener:

> The experimental method, upon which all science rests, is, logically considered, a method of the induction of a generalized correlation by means of controlled concomitant variations. In the simplest experiment there are always at least two terms, an independent variable and a dependent variable. The experimenter varies *a* and notes how *b* changes, or he removes *a* and sees if *b* disappears. He repeats until he is satisfied that he has the generalization that *b* depends upon *a*. The independent variable, *a*, can now properly be spoken of as a *cause* of the dependent variable, *b*. (pp. 8–9)

Boring gave no source for this formulation. A search of correspondence between Boring and Woodworth at both the Harvard and Columbia University archives failed to yield any discussion of this issue, although there is correspondence regarding many other organizational and professional issues. It is not surprising that two leading figures would make a nearly simultaneous change in language. However, they were not the only important actors here. Danziger (1997; Danziger & Dzinas, 1998) described how Edward C. Tolman also introduced the concept of independent variables.

In his *Purposive Behavior in Animals and Men*, Tolman (1932) emphasized the language of "variables" when summarizing different systems of psychology in Chapter XXIV, "The Final Variables of Purposive Behaviorism." What is important here is that the term *variables* allowed Tolman to contrast individual difference psychology, structuralism, Gestalt psychology, and behaviorism using a common language. I have argued previously that Tolman did not quite use the Boring/Woodworth formulation in 1932, and that he used an idea of "independent causes" which were clearly variables, without saying independent variables. This assertion was not correct: on page 405, Tolman referred to heredity and previous training as "two independent variables." On page 406–407 it is clear that Tolman uses the terms "independent causes" and "independent variables" interchangeably. As Danziger (1997) has argued, this is the most important feature of the introduction of variables: the term is used to describe not just a methodology but the theoretical entities of interest. Moreover, method and theory subsequently take on an isomorphism uncharacteristic of discourse in the natural sciences.

In 1935, Tolman (1932/1967) wrote in "Psychology and Immediate Experience" that both molecular (physiological) and molar behaviorists shared a common program of identifying the "independent or causal variables" (p. 102). He summed up his integrative position:

> A behaviorism seeks to write the form of the function f_1 which connects the dependent variable-the behavior, B-to the independent variables-stimulus, heredity, training, and physiological disequilibriums, S, H, T, and P. (p. 113)

Thus Tolman emphasized the concepts of independent variables and dependent variables as a way of defining the project of psychology, and as the proper way to conceptualize the theoretical terms of interest. In addition, these terms aided in the explication of his "intervening variables," which was a crucial move in the

synthesis he attempted through Purposive Behaviorism. Unlike Woodworth, he did not present these terms as a way of defining what an experiment is, but as a way of defining what an explanation of behavior would look like.

There can be little doubt that Tolman's continued use of these terms helped in their popularization. His APA presidential address of 1937, published as "Determiners of Behavior at a Choice Point" in the *Psychological Review* the next year (Tolman, 1938), the terms *independent* and *dependent variable* were featured very prominently (in figures, 2, 3, 6, 7, 8, 12, & 15) and the function relating them was said to provide the "cause." It is interesting that by this time, the term *cause* had been allowed back into discussions of function, despite Ernst Mach's attempts to eliminate this idea as freighted with metaphysical baggage (Winston, 2001). Insofar as Tolman's position provided a unified vision for psychological inquiry, providing an analysis of the problems of behaviorism, Gestalt psychology, individual psychology and purposive psychology, Tolman's language of independent variables, intervening variables, and dependent variables allowed psychologists of varied commitments to speak a common scientific language.

Tolman was teaching at Harvard during the summer of 1932, and Skinner (1998) reported that he "saw a great deal of him" (p. 290) although Tolman (1952) did not mention this absence from Berkeley in his autobiography. Skinner suggested that he and Tolman were speaking about variables in a similar way at this time. Skinner's statement of behavior as a function of a set of variables was, he thought, related to that of Tolman. However, Skinner rejected intervening variables in favor of "third variables," such as deprivation, which altered the functional relationship between stimulus conditions and responding.

Skinner's (1998) retrospective account suggests that he had priority in the modern use of "variables." In Skinner (1931) he argued that the experimental results of the study of a reflex can be expressed as the function: $R = f(S)$, or considering third variables, $R = f(S, A)$. He treated this relationship as experimentally derived by manipulation, and not merely as the statistical relationship between two sets of numbers (pp. 451–452). He did not use the terms *independent variable* and *dependent variable*, although they are clearly implied by his formulation.[4] Moreover, Skinner's philosophy of science, so heavily influenced by Mach (see Winston, 2001), emphasized the determination of functional relationships as the aim of his science of behavior.

Priority claims are hardly the important issue here. What is significant is the social matrix in which such changes in language occurred and the social process by which they became codified and enshrined. Woodworth, Boring, and Tolman occupied positions of influence in the APA, by then a rapidly growing organization. Skinner was a new PhD, but outspoken and confident in his views. Although Boring found Skinner difficult and had severe criticisms of Skinner's dissertation (see Bjork, 1993), he respected his intellectual talents and promoted Skinner's career in a variety of ways. Skinner was able to publish his new conceptualization

of the reflex with little delay, with the help of William Crozier, who was an editor of the *Journal of General Psychology*. Boring and Tolman were also in frequent contact, as Boring was attempting to recruit Tolman to return to Harvard, where he had received the PhD in 1915 (Innis, 1992). There were ample opportunities for discussion of general theoretical issues amongst these four important individuals. They in turn had numerous opportunities to encourage the formulation of all psychological problems in terms of *independent* and *dependent variables*. I do not mean to imply that a cabal was formed with any explicit plan to alter the language of psychologists, only that in conversation these individuals may have discovered collective as well as individual reasons for adopting and promoting a new discourse.

FAILURE TO CHANGE METALANGUAGE: SAUL ROSENZWEIG AND *Ee*

In order to understand the process by which a new linguistic practice is introduced and taken up, it may help to consider cases in which attempts to change scientific language *fail*. One clear attempt occurred in 1933, and provides an instructive example in the microhistory of the regulation of language. In contrast to the introduction of *independent variable* and *dependent variable* at the same time, where no archival record of the relevant correspondence has been found, the discussions regarding this attempt to change the metalanguage are available, and can clarify the role of power and status in bringing about or inhibiting such change.

Saul Rosenzweig (1907–) became a graduate student at Harvard in 1929 and received the PhD in 1932. He had some difficulty obtaining an academic position, despite an excellent record, due in part to the Depression and possibly to antisemitism (see Winston, 1998). He continued his work at the Harvard Clinic with Henry Murray. Boring (1932) described him as "top-notch intellectually" and "our best graduate this year." In 1933, Rosenzweig published an important and often neglected paper: "The Experimental Situation as a Psychological Problem." Some forty years before the topic became fashionable, he introduced the idea that the experiment must be considered a *social situation* in which the elements were quite different from an experiment in chemistry. As a conscious being, the person serving as "subject" may "regulate their own reactions: they have minds of their own and are self critical. From this results the difficulty that uncontrolled experimental materials, *viz.*, motives, may be brought into the experiment" (1933a, p. 353). Rosenzweig suggested that the subjects might engage in playing the part or role they thought was expected of them, might attempt to appear smart or compliant, would develop hypotheses about what the experimenter was after, and would generally behave in an active rather than passive fashion. He was concerned that neither the term *subject*, which had only recently become standard, nor the older

term *observer*,[5] captured this aspect of the person participating in an experiment. Rosenzweig therefore introduced a new term, *experimentee* or *Ee*, to be used along with *Er*, rather than *E* for the experimenter. At the beginning of the article he noted:

> The word > 'experimentee' is the exact equivalent of the German > 'Versuchsperson.'
> It is a general designation that may be used to refer to the person, whatever his
> function (that of observer or that of subject) working in coöperation with, but in a
> complementary relation, to the experimenter. It is an authorized English word and it
> is suggested that it be more commonly employed. (p. 338, n. 1)

The same year, Rosenzweig used the term *experimentee* in two additional publications. In the *Journal of Genetic Psychology*, Rosenzweig (1933d) examined children's preferences for repeating successful and unsuccessful activities. In the first footnote, he argued for the equivalence of Ee and *Versuchsperson*, noting that "our Ees were not strictly 'subjects,' in the sense of behavioral animals, nor were they 'observers' . . . They were in part both and the term 'experimentee' covers this general function" (p. 423). Rosenzweig and Koht (1933) used the same term in an article on time estimation, with a briefer footnote on *Versuchsperson*.

E. G. Boring, head of the Laboratory and effective head of the not-yet-independent Department, did not like this change. Rosenzweig, who distinctly recalls the interchange with Boring in 1933, reported that Fernberger asked Boring whether he had approved of the use of *experimentee*, which led Boring to intervene (personal communication, July, 2002). Boring and Rosenzweig began a discussion of the matter. Boring (1933a) wrote:

> I don't like Er and Ee at all. They're just your private substitutes for *E* and *S*, which
> have after some difficulty gotten themselves accepted quite generally in the rapidly
> expanding American Literature. For you now to attempt substitutes is unfortunate,
> from your point of view because you have to overthrow tradition and are likely there-
> fore to be unsuccessful after much effort, from the public's point of view because any
> degree of incomplete success by you leads to confusion, ambiguity, and distraction
> of attention from the things that really matter.

Boring went on to give a history of the terms *E*, *S*, and *O*, and argued that *S* was now the general term that encompassed both introspecting and non-introspecting organisms. He could see no reason at all to change *E* to *Er*. Boring emphasized the issues of clarity and confusion, and chastized Rosenzweig:[6]

> We had enough trouble getting these things established fifteen years ago, more or
> less; but now you want to buck tradition. (Or don't you?). Moreover, the public was
> represented in 1920 by 400 members of the APA, whereas now it is represented by
> 1600 members. If we felt the inertia then, think how much more you will feel the
> inertia now.

He suggested that journal editors ought to change *Ee* and *Er* back to *S* and *E*, to avoid confusion. This emphasis on harmony is, I believe, central to understanding the way things looked to Boring.

Rosenzweig (1933b) wrote back a few days later, mentioning his recent meeting with Boring on the matter and enclosing a six page defense of *Er* and *Ee*. Here he outlined the ambiguities of the term *subject* and the complex functions served by the person "experimented on." He attempted to answer a number of Boring's objections, including the awkwardness of *Ee*, and compromised by suggesting the *Er* could stay as just *E*, but that a designation for *experimentee* was still necessary. Three days later, Boring (1933b) replied that he remained unconvinced. With his characteristic thoroughness, perhaps to the point of obsessiveness, Boring endeavored to show Rosenzweig that the term *subject* was the term in general use no matter what precise role the person assumed. He surveyed 49 articles to demonstrate this assertion empirically. Inviting Rosenzweig to reply, perhaps with an expanded sample, he proposed to circulate a memo to Howard Warren, Samuel Fernberger, Henry Murray, and the *American Journal of Psychology* editorial board explaining that *Ee* and *Er* were "unnecessary redundant terms" and should not be used.

Another note to Rosenzweig followed three days later on December 22. Now Boring (1933c) reported that he had already discussed the *Ee* and *Er* issue with Henry Murray (who was Rosenzweig's ostensible "boss") over lunch, and "you can ask Dr. Murray what he thinks the result was." Apparently, Murray thought that Boring was pressuring Rosenzweig to retract his use of Ee and Er, and Boring felt obliged to tell Rosenzweig that this was not his intent. Boring, who imposed extremely strict rules on himself, would not let it be thought that he would bully anyone. Rosenzweig (1933c) replied on the 26th. He politely thanked Boring for his careful analysis, but he refused to accept Boring's position "in an unqualified way." That is, he agreed that *subject* was used as a general term, but argued now that use of *experimentee* would avoid the behavioristic connotations that *subject* had acquired. While defending his proposal, he acknowledged that using *experimentee* might "arouse needless antagonism on the part of the reader, cause confusion, and distract attention from more important aspects of any publication containing this usage." Characterizing the action as his own decision, he reported that he would now use *subject* rather than *experimentee*, and had already made the change in his next publication in the *British Journal of Psychology* (Rosenzweig & Mason, 1935). He thanked Boring for showing him that "established usage (even if inconsistent) cannot be ignored with impunity," and that antagonism or confusion of the reader was not worth the possible gain.

Boring (1933d), who never took a holiday from his correspondence, wrote back on December 29:

You have yielded handsomely to my judgment and have simplified my editorial problem greatly. I am circulating this memo to you, Murray, Warren, Fernberger and my three colleagues on the AJP. I discussed the matter with Warren and Washburn Wednesday evening. Of course I put a prejudiced case, but so far I have not found anybody who sees so much advantage in *Ee* and *Er* as to be willing to affirm that belief to me. Enough.

In politely but very firmly bringing this disagreement to an end, Boring exercised considerable authority. It was important for him to bring Fernberger and Murray on side, as Fernberger had somehow let Rosenzweig's use of Ee stand in the *Journal of Experimental Psychology*, and Murray had recommended publication of Rosenzweig (1933d) in the *Journal of Genetic Psychology*. Rosenzweig did not forget this blocking of his aim to bring the social relationship between experimenter and subject to the attention of psychologists. In his obituary for Boring, Rosenzweig (1970) devoted considerable attention to Boring's use of *Zeitgeist*, and described the events of 1933:

> . . . I reluctantly recall a personal encounter with the *Zeitgeist* as represented by Boring himself . . . I had introduced the term *experimentee* as inclusive of both *subject* and *observer* (a term still used in those days) but found myself thwarted by a protracted campaign of opposition from him, by letter, interview, and memorandum, that eventuated in my diplomatic acceptance of the Department Chairman's judgment. (1970, p. 68)

Rosenzweig felt in 1933, 1970, and 2002 (personal communication) that the issue was not just the proper use of a novel term, but that the term signalled a different understanding of the nature of psychological experimentation from that which prevailed in 1933. The idea of Boring himself as the embodiment of the *Zeitgeist* remains intriguing. But it was Boring who took the necessary action. As a result, Rosenzweig acquiesced and never used the terms again.

The case of *experimentee* illustrates the way in which the regulation of language in this academic context was carried on in explicit, active, fashion, rather than through implicit understanding. Had Boring endorsed Rosenzweig's proposal, additional communications to his network of editors and leading figures in Psychology would have undoubtedly gone out. Although formal instructions to authors on the preparation of manuscripts in psychology first appeared in 1929 (see Vandenbos, 1992), the power of those such as Boring to persuade or even command was formidable. He was able to immediately forestall further use of Rosenzweig's terms in the *Psychological Review*, *Psychological Bulletin*, *Journal of Experimental Psychology*, the *American Journal of Psychology*, and other outlets. As opposed to the formal gatekeeping of journal editors (see Lubek & Apfelbaum, 1987), Boring's power was more widespread. It is tempting to think of Boring's network in terms of the "actor-network-theory" relating scientists, engineers, politicians, texts, and devices, as described by Latour and others (e.g., Latour, 1987, 1999).[7] Although the *social* network surrounding Boring is much more limited in scope and different in character than the *asocial* system of technoscience conceived by Latour, the process by which colleagues and texts are enlisted in disputes according to actor-network-theory seems an apt description of the Rosenzweig case. Specifically, Boring's report of his discussions with Henry Murray and Howard Warren, and his employment of an informal survey regarding

the use of "subjects" illustrates the social production of authority through texts as outlined by Latour.

Boring's choice to promote the definition of experiment in terms of *independent* and *dependent variables* but reject the term *experimentee* was hardly arbitrary. In the former case, the language was a means to speak of diverse practices and conceptions, from Titchenerian structuralism to Watsonian behaviorism, in a common voice. In the case of *experimentee*, Boring foresaw confusion and division, rather than the Peaceable Kingdom of Psychology that both he and Woodworth hoped for.[8] Moreover, Rosenzweig's attempt to alert psychologists to the uniquely social nature of experimentation with humans was potentially problematic for a unified, natural science view of human and animal psychology.

SPREADING THE LANGUAGE OF INDEPENDENT VARIABLES

The appearance of the standard conception of independent variables, experiment, and causality in the discourse of Boring, Woodworth, and Tolman did not ensure that this formulation would become universal, nor was the adoption of their language an immediate consequence. Uniformity of metalanguage was not achieved until the 1960s or 1970s.

Between the 1930s and the 1970s, the concepts of independent and dependent variables appeared with increasing frequency in psychology textbooks and the psychology literature. As documented by Winston and Blais (1996) in a sample of texts from Psychology, Sociology, Biology, and Physics, this development was initially unique to Psychology, although Sociology texts began to use these terms in the 1970s. The change in Psychology was accompanied by a narrowing of the definition of experiment to include only studies in which the independent variable was explicitly manipulated, and the frequent assertion that only such studies would reveal causal relationships. This formulation appeared to serve important pedagogical aims: all psychological research could be formulated with a common framework, easily understandable even for introductory level students. This smoothing over of disagreement by means of a common emphasis on method rather than theory meant that textbooks could present the appearance of a unified and coherent discipline. Such coherence would make the introduction of psychology to a mass audience an easier task.

Introductory textbooks are now of interest in their own right, as important records of the changing relationship between scientific authors and nonscientific audiences (see Morawski, 1992, 1996; Weiten & Wight, 1992). However, it is unlikely that the introductory textbook played a major role in guiding how psychological research was formulated after the 1930s. Texts for Experimental Psychology courses are likely to be more important than introductory texts in socializing new recruits to the discipline; as students begin the active participation in "laboratory
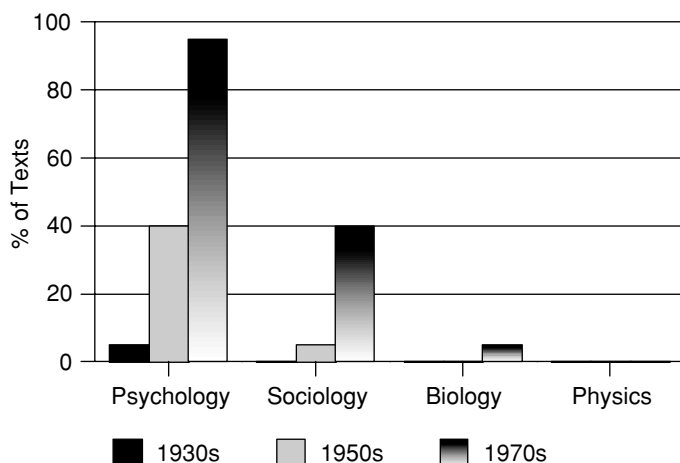
FIGURE 1. % of texts using the terms "independent variable" and "dependent variable" by discipline. Adapted from Winston & Blais, 1996.

life," the sense of a new identify can be formed. Although not a laboratory manual, Robert S. Woodworth's *Experimental Psychology*, known as the "Columbia Bible" played a crucial role (Winston, 1990). In the widely used "Bible," Woodworth clearly demarcated experimental from correlational research in terms of whether or not variables were actively manipulated, and assigned the discovery of cause and effect to experiments only. This discursive move gave experimentation a distinct epistemological advantage, and thereby elevated experiments to the premier form of knowledge generation.

With the exception of Woodworth (1938), experimental psychology textbooks from the 1930s gave no definition or only a vague definition of experimentation, and did not use the terms *independent* and *dependent* variables. Most frequently, experiment was defined as introspection via controlled self-observation or simply controlled observation, and the requirement of manipulation was generally not mentioned. The shift in definition is clearly evident but still not universal by the 1950s. One text in particular stands out as transitional: Underwood's (1949) highly influential *Experimental Psychology*, which explicitly used *independent* and *dependent variables* and defined true experiments as requiring manipulation.[9] Underwood's text may have been the first to devote a full chapter to explaining just what an experiment is supposed to be. Moreover, this text is very different from all previous ones: in older texts, "experimental psychology" is used to designate a collection of topics as well as a method; the focus is on the content of the discipline. Underwood's focus was more on a universally applicable method and less on content.

Postman and Egan (1949) published a similar text in the same year, and they also followed the Boring/Woodworth definition of experiment. But not all did.

In *Beginning Experimental Psychology*, Bartley (1950) noted that "The concept of what experimentation is and what constitutes an experiment varies considerably from person to person" (p. 30), cited Bentley's (1937) discussion of eight different definitions of experiment (see Winston 1990), and offered no resolution. The diversity on such a fundamental issue suggests that the textbook uniformity was by no means a certain outcome. Of 15 experimental psychology texts from the 1970s that I examined, all but one used the contemporary definition in terms of manipulation of an independent variable. In the 1950s, there is little mention of non-experimental methods that psychologists might use. But in the 1970s, experimentation is compared and contrasted with other approaches, particularly "correlational" strategies. Observation, correlation, and experimentation are presented in that order, with hints that experimentation is historically the most recent method and epistemologically the most highly developed. Experiments are described as the final step in systematic inquiry, whereas observation and correlation are preliminary steps. A similar shift can be seen in Social Psychology textbooks and handbooks (MacMartin & Winston, 2000). In some cases, the textbooks or handbooks in Social Psychology made explicit reference to Underwood (1949) or Woodworth (1938) as authorities on the precise definition of experiment. In contrast to the direct exercise of power described in the case of Rosenzweig and Ee, the influence here is likely to be informal and indirect. Woodworth, Boring, and Tolman had substantial prestige within the large network of Columbia and Harvard PhD's, and their discursive practices were both respected and imitated.

These textbook changes were accompanied by changes in the rhetoric in journal articles. Danziger and Dzinas (1997) examined the use of the terms independent and dependent variables in the *Journal of Experimental Psychology, American Journal of Psychology, Journal of Personality* and the *Journal of Abnormal and Social Psychology*. The percentage of articles using these terms rose from 0% in 1938 to 2% in 1948 to 14.6% in 1958. Surprisingly, the *Journal of Abnormal and Social Psychology* showed the most frequent use, i.e., 27% of articles in 1958. Danziger and Dzinas suggested that in those areas where experimentation was not as well established, there might exist a stronger inclination to affirm this new methodological commitment with explicit terminology. The period from the late 1940s to the early 1960s saw the rapid transformation of American Social Psychology from a methodologically diverse to methodologically homogeneous enterprise.[10] The proportion of articles in the *Journal of Abnormal and Social Psychology* (later the *Journal of Personality and Social Psychology*) with explicit manipulation of variables rose from less than 30% in 1949 to over 80% by 1959 (Christie, 1965; Higbee & Wells, 1972). In contrast to traditional experimental areas such as perception where the use of experimentation was never an issue, this transformation in Social Psychology required explicit demarcation of experiment and its significance.
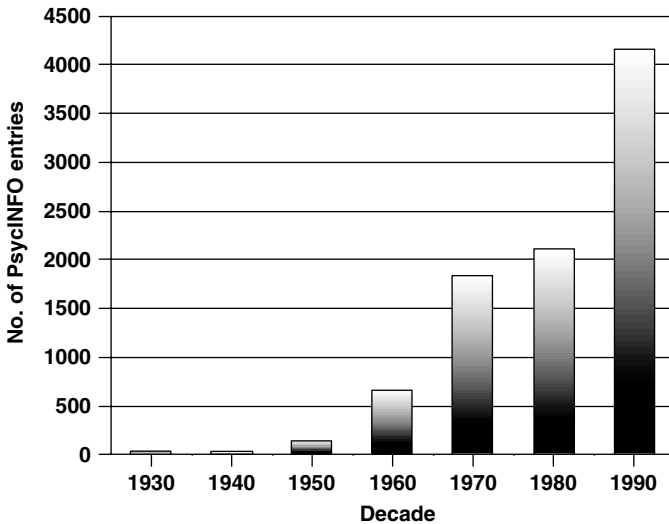
FIGURE 2. Number of PsycINFO entries using the terms "independent variable" or "dependent variable" by decade.

The more general spread of the terms *independent* and *dependent variable* can also be seen in the PsycINFO database. The number of abstracts using either term increases rapidly after the 1960s, rising to over 4100 in the 1990s, as shown in Figure 2.[11] However, these data must be viewed in context: 4000 abstracts represents less than 1% of the 1990s database. In most cases, it would be unnecessary for the authors of journal articles to identify the manipulated variable as the independent variable. For Woodworth, these terms were primarily a pedagogical device for the uninitiated. Their use helped to achieve the clarity that Woodworth was known for in his textbooks. For the undergraduate psychology major, hoping to enter graduate school, adherence to and demonstration of the appropriate metalanguage of method would be demanded in all laboratory reports and examinations. Having been taught to formulate psychological investigations in these terms beginning with introductory psychology, the newly minted researcher would need only to state that the problem under study was "the effect of x on y"; the journal audience, all trained in experimental psychology, knew which was which.

The nearly universal promotion of the view that causality could only be discovered by manipulation of the independent variable had important consequences for the investigative practices of psychology. Such a view helped delegitimize nonexperimental models for scientific psychological inquiry, such as the natural history approach proposed by Gardner and Beatrice Murphy in the 1930s (Pandora, 1997). At the very least, naturalistic observation would be relegated to inferior epistemological status, as would correlational studies. Second, promotion

of psychology as the only social science to use experimentation helped establish a distinct disciplinary identity resting on the claim that psychology could identify the causes of human action while other, nonexperimental human sciences could not. Third, the requirement of manipulation and control for causal inference helped to justify the choice of laboratory animals as subjects in that animals would permit the degree of manipulation and control necessary for such inference. These requirements would also encourage the use of other "captive" populations, including undergraduates required to fulfill a requirement in introductory courses. Finally, the reduction of human experience and social interchanges to a set of manipulated variables provided a justificatory rhetoric for the laboratory study of attitude change, aggression, competition, moral development, and other socially embedded phenomena in the ahistorical, acultural, and decontextualized approach common from the 1950s through 1980s.

Danziger (1997) identified another critical aspect of the shift in metalanguage. What changed, he argued, was not simply the terminology or definition of experiment. The *independent variable* changed from being a statistical or procedural concept to standing for the theoretical entity of interest. That is, a variable in an experiment came to be treated as "an objective natural force with causal efficacy" (Danziger, 1997, p. 172). This shift in ontological status describes how "variables" in general came to be used in psychology, not just in experimentation. Thus "anxiety" was transformed into much more than a statistical aggregate of responses to questionnaire items, and was assumed to be a variable operating in the world, discovered rather than created. Moreover, the assumption that psychological reality was pre-structured as variables generally went unexamined, becoming a part of the taken-for-granted in psychological research. For Danziger, this treatment of variables shaped the way in which theoretical as well as empirical statements were constructed.

The general acceptance of the view that manipulation of the independent variable was the sole path to understanding the causes of behavior was enabled and supported by the intertwined discourses of positivism, neo-behaviorism, and operationism that existed in many variations in the 1930s (see, e.g., Green, 1992, Smith, 1986). Moreover, the spread of the new language was embedded in the development of other methodological and technical concepts and tools, such as the idea of randomization and experimental control (Dehue, 2000, 2001) and the spread of analysis of variance (Danziger, 1997; Rucci & Tweney, 1980). Both Danziger and I have emphasized the relationship of changes in investigatory practices to the demands of applied psychological work and the adoption of a technological view of science. Van Hoorn (1983) argued that the triumph of a technological approach to psychology and other social sciences was greatly accelerated after World War II, and was tied to aims of social control. The history of these complex interrelationships is beyond the scope of this chapter. However, consensus regarding experiment, functions, and variables should be seen as part

of the longer history, beginning at least with the fundamental epistemological split described by Danziger (1979) in "The positivist repudiation of Wundt." In one of his earliest historiographic papers, Danziger illuminated the critical turning point when Külpe, Ebbinghaus, and Titchener all adopted a Machian philosophy of science, in which functions and variables would come to play a crucial role.

CONCLUSIONS

The success of the Boring/Woodworth definition of experiment, and the failure of Rosenzweig's social conception of *Experimentee* during the 1930s both illustrate the regulation of scientific language through power and prestige. Both of these innovations in the metalanguage of Psychology were more than a change in terminology, and offered accompanying versions of the meaning of an experiment. The language of *independent* and *dependent variables* promised to reveal causal relations and unify the discipline under a methodological rather than theoretical hegemony. The aim of using *Experimentee* was to bring recognition of the experiment as a fundamentally social encounter and to change the conception of the subject from a passive recipient of stimuli to an active being, an agent capable of reformulating or even defeating the experiment.[12] The two proposals had an important asymmetry: one offered harmony by smoothing over epistemological debate, the other pointed toward confusion over just what was happening in psychological investigations. The steps necessary to promote one and prohibit the other were also asymmetrical: Rosenzweig's proposal was "terminated" within a matter of weeks via direct action, while the new definition of experiment spread gradually and "naturally" over several decades.

E. G. Boring accomplished the swift death of Rosenzweig's proposal through the exercise of institutional power, professional status, and personal contacts. His network of relationships was based on long personal histories of interaction and professional interchange. Moreover, Boring's vast correspondence files suggest that he functioned as a very central "node" in this network, through which news, policies, job postings, recommendations, and other discursive materials flowed. Careful consideration of such networks is a useful corrective to a historiographically oversimplified narrative, in which Boring's individual will triumphs over that of Rosenzweig.

In contrast to the Rosenzweig case, the gradual spread of the standard conception of experiment, variables, and causality was accomplished without the dramatic intervention of powerful individuals, although powerful individuals introduced the change. Boring, Woodworth, and Tolman were interconnected by their commitment to laboratory experimentation, to a psychology of basic principles rooted in their vision of natural science, and to the APA and the Society for

Experimental Psychologists. The textbook authors who promoted these conceptions were also interconnected. In the 1920s and 1930s, graduates of Columbia University were strategically placed in departments around the country, and former students typically remained in close contact with their mentors. As younger psychologists produced both introductory and experimental psychology textbooks, they would naturally draw on the work of Woodworth, particularly the "Bible." The resulting intertextuality (see Culler, 1981) should not be surprising. There is no need to search for missing documents to explain the uniform presentation of method.

It is customary, as I have done here, to speak of such changes in language in terms of "spread," as in the spread of weeds, fire, or disease, all highly unsatisfying metaphors. However, the vagueness of "spread" may be remedied in two ways. First, in *Naming the Mind*, Danziger outlined the functions served by the new talk of variables. He provided a cogent account of what new activities and positions were enabled by this move, and used the example of the Harvard Psychological Clinic to demonstrate how the language of variables provided unity in a climate of faction and feud. Second, change in the discursive practices of psychologists can be situated within a more general cultural history. In this chapter, I have neglected the dramatic post—WWII expansion of Psychology that accompanied the "spread" of the language of independent variables. The increased federal funding, societal interest, political and social applications, and pressure for scientific legitimization provide an important context in which to view mirco-level changes in the language of psychology (e.g., see Capshew, 1999; Herman, 1995).

Although the postwar environment is crucial for understanding stability and change in these discursive practices, the context of 1930s psychology must also be considered. The unresolved tensions within the discipline were considerable: conflicts between psychologists in the laboratory and growing numbers in applied work, between the activist founders of SPSSI and those who resisted such a role for psychology, between the un- or underemployed and the well established.[13] Despite the ravages of the Great Depression, the APA continued to grow rapidly during the 1930s, changing from an exclusive club of the like-minded toward a mass organization of competing factions. In this context of intradisciplinary discord, tight control over the language of experimentation was essential for creating the dominance of neo-behaviorism and the laboratory study of animal learning in the subsequent decades.

NOTES

[1] Although the abstract for Meakin (1904) contains "independent variables," the phrase does not appear in the original article. The PsycINFO abstracts for this period were created recently (see PsycINFO News, 1996), and Meakin (1904) does not constitute an early use of "independent variable" in the psychological literature.

[2] In James, the relevant passage is a discussion of whether cortical ablation in dogs produces motor disorders as a result of sensory anesthesia, or "independent" of the sensory deficit. James concluded from studies showing motor effects without sensory effects that "The motor and sensory symptoms seem, therefore, to be independent variables" (p. 59).

[3] For a negative view of Jevons' philosophy of science, see Pearson's (1892) *Grammar of Science.*

[4] Skinner used the term "dependent variable" in *Behavior of Organisms* (Skinner, 1938), but did not emphasize the formulation of his behaviorism in these terms until *Science and Human Behavior* (Skinner, 1953).

[5] The use and decline of the term "observer" is discussed in Danziger (1990).

[6] Chastisement by Boring for one infraction or another was a fairly common occurrence for Harvard graduate students. See, Winston (1998) for examples.

[7] This approach is sometimes known as "actant-network" theory, with the term "actant" instead of "actor" emphasizing the active role of texts, devices and other objects as "actors." The founders of ANT have declared the movement "over" or "dead" (see Law & Hassard, 1999) although this claim can be taken as ironic.

[8] Quaker minister Edward Hicks (1780–1849) painted a large number of works depicting the "Peaceable Kingdom" of Isaiah. Boring was raised in a part Hicksite (founded by Edward Hicks' cousin Elias Hicks) and part Orthodox Quaker family, and Quaker values had a substantial influence on him (Winston, 1998). This did not mean that Boring was accepting of innovation.

[9] For information on Underwood and his influence see Freund (1998).

[10] See Lubek (2000) for detailed discussions of the transformation of Social Psychology.

[11] All documents in PsycINFO, not just journal articles, were included in this survey.

[12] To an extent, Rosenzweig's conception was incorporated into the Fourth Edition of the APA *Publication Manual* 61 years later (APA, 1994). Recommending that in most cases, *participant* or other terms be substituted for *subject*, the *Manual* also mandated an end to passive language for discussing participants, and required terms that described them as actors rather than acted upon.

[13] See, for example, Finison (1976), Harris, Unger, & Stagner (1986), O'Donnell (1979), and Samelson (1992).

ACKNOWLEDGMENT

REFERENCES

American Psychological Association (1994). *Publication manual of the American Psychological Association*, 4th Edition. Washington, DC: American Psychological Association.

Bartley, H. (1950). *Beginning experimental psychology*. New York: McGraw-Hill.

Bentley, M. (1937). The nature and uses of experiment in psychology. *American Journal of Psychology, 50*, 252–269.

Bjork, D. (1993). *B. F. Skinner: A life*. New York: Basic Books.

Boring, E. G. (1932, March 21). Letter to Herbert Woodrow. E. G. Boring Papers, Harvard University Archives.

Boring, E. G. (1933a, December 8). Letter to Saul Rosenzweig. E. G. Boring Papers, Harvard University Archives.

Boring, E. G. (1933b, December 19). Letter to Saul Rosenzweig. E. G. Boring Papers, Harvard University Archives.

Boring, E. G. (1933c, December 22). Letter to Saul Rosenzweig. E. G. Boring Papers, Harvard University Archives.

Boring, E. G. (1933d, December 29). Letter to Saul Rosenzweig. E. G. Boring Papers, Harvard University Archives.

Boring, E. G. (1933e). Physical dimensions of consciousness. New York: Century.

Capshew, J. (1999). *Psychologists on the march: Science, practice, and professional identity in America, 1929–1969.* New York: Oxford University Press.

Christie R. (1965). Some implications of research trends in social psychology. In O. Klineberg & R. Christie (Eds.), *Perspectives in social psychology* (pp. 141–152). New York: Holt, Rhinehart and Winston.

Culler, E. (1927). The accuracy of the method of constant stimuli. A reply to Dr. Urban. *American Journal of Psychology, 38*, 307–312.

Culler, J. (1981). *The pursuit of signs: Semiotics, literature, deconstruction*. London: Routledge & Kegan Paul.

Danziger, K. (1979). The positivist repudiation of Wundt. *Journal of the History of the Behavioral Sciences, 15*, 205–230.

Danziger, K. (1987). Statistical method and the historical development of research practice in American psychology. In G. Gigerenzer, L. Kruger and M. Morgan (Eds.), *The probabilistic revolution, Volume 2: Ideas in modern science* (pp. 35–47). Cambridge, MA: MIT Press.

Danziger, K. (1990). *Constructing the subject: Historical origins of psychological research*. New York: Cambridge University Press.

Danziger, K. (1997). *Naming the mind: How psychology found its language*. Thousand Oaks, CA: Sage.

Danziger, K., & Dzinas (1997). How psychology got its variables. *Canadian Psychology, 38*, 43–48.

Dehue, T. (2000). From deception trials to control reagents: The introduction of the control group about a century ago. *American Psychologist, 55*, 264–268.

Dehue, T. (2001). Establishing the experimenting society: The historical origin of social experimentation according to the randomized controlled design. *American Journal of Psychology, 114*, 283–302.

Finison, L. (1976). Unemployment, politics, and the history of organized psychology. *American Psychologist, 31*, 747–755.

Fisher, R. A. (1925). *Statistical methods for research workers*. London: Oliver and Boyd.

Freund, J. S. (1998). Benton J. Underwood: A tribute of memories. In G. A. Kimble & M. Wertheimer (Eds.), *Portraits of pioneers in psychology, Vol. III.* (pp. 309–328). Washington, DC: American Psychological Association.

Green, C. (1992). Of immortal mythological beasts. *Theory & Psychology, 2*, 291–320.

Harris, B., Unger, R. K., & Stagner, R. (Eds.) (1986). Fifty years of psychology and social issues. *Journal of Social Issues, 42*, No. 1.

Heidbreder, E. (1927). Introversion and extraversion in men and women. *Journal of Abnormal & Social Psychology, 22*, 52–61.

Herman, E. (1995). *The romance of American psychology: Political culture in the age of experts.* Berkeley: University of California Press.

Higbee, K. L. & Wells, G. (1972). Some research trends in social psychology during the 1960s. *American Psychologist, 27*, 963–966.

Innis, N. K. (1992). Tolman and Tryon: Early research on the inheritance of the ability to learn. *American Psychologist, 47*, 190–197.

James, W. (1950). *Principles of Psychology, 2 Vols.* New York: Dover. (Original work published 1890.)

Jevons, W. S. (1874). *Principles of science: A treatise on logic and scientific method.* London: MacMillan.

Latour, B. (1987). *Science in action: How to follow scientists and engineers through society.* Cambridge, MA: Harvard University Press.

Latour, B., (1999). On recalling ANT. In J. Law and J. Hassard, (Eds.), *Actor network theory and after* (pp. 15–25). Oxford: Blackwell Publishers/ The Sociological Review

Law, J. & Hassard, J. (Eds.) (1999). *Actor network theory and after.* Oxford: Blackwell Publishers/ The Sociological Review.

Lubek, I. (Ed.) (2000). Re-engaging the history of social psychology. Special Issue, *Journal of the History of the Behavioral Sciences, 36*, no. 4.

Lubek, I., & Apfelbaum, E. (1987). Neo-behaviorism and the Garcia effect: a social psychology of science approach to the history of a paradigm clash. In M. G. Ash & W. R. Woodward (Eds.), *Psychology in twentieth century thought and society* (pp. 59–91). Cambridge: Cambridge University Press.

MacMartin, C. & Winston, A. S. (2000). The rhetoric of experimental social psychology, 1930–1960: From caution to enthusiasm. *Journal of the History of the Behavioral Sciences, 36*, 349–364.

Meaken, F. (1903). Mutual inhibition of memory images. *Psychological Monographs, 4*, 235–275.

Morawski, J. G. (1992). There is more to our history of giving: The place of introductory textbooks in American psychology. *American Psychologist, 47*, 161–169.

Morawski, J. G. (1996). Principles of selves: The rhetoric of introductory textbooks in American psychology. In C. F. Graumann & K. J. Gergen (Eds.), *Historical dimensions of psychological discourse* (pp. 145–162). New York: Cambridge University Press.

O'Connor, J. J. & Robinson, E. F. (2002). Johann Gustave Peter Lejeune Dirichlet. *Mactutor History of Mathematics Archive*, University of St. Andrews, St. Andrews, Scotland. Accessed June 15, 2002 at: http://www-history.mcs.st-andrews.ac.uk/history/Mathematicians/Dirichlet.html

O'Donnell, J. M. (1979). The crisis of experimentalism in the 1920s: E. G. Boring and his uses of history. *American Psychologist, 34*, 289–295.

Pandora, K. (1997). *Rebels within the ranks: Psychologists' critique of scientific authority and democratic realities in New Deal America.* Cambridge: Cambridge University Press.

Pearson, K. (1892). *The grammar of science.* London: J. M. Dent & Sons.

Postman, L. & Egan, J. P. (1949). *Experimental psychology: An introduction.* New York: Harper.

PsycINFO News. (1996). New retrospective database to foster research in the History of Psychology, *PsycInfo News, 16*, no. 1. P. 1. Accessed at www.apa.org, June 15, 2002.

Rosenzweig, S. (1933a). The experimental situation as a psychological problem. *Psychological Review, 40*, 337–354.

Rosenzweig, S. (1933b, December 16). Letter to E. G. Boring. E. G. Boring Papers, Harvard University Archives.

Rosenzweig, S. (1933c, December 26). Letter to E. G. Boring. E. G. Boring Papers, Harvard University Archives.

Rosenzweig, S. (1933d). Preferences in the repetition of successful and unsuccessful activities as a function of age and personality. *Journal of Genetic Psychology, 42*, 423–441.

Rosenzweig, S. (1970). E. G. Boring and the Zeitgeist: *Eruditione gesta beavit. Journal of Psychology, 75*, 59–71.

Rosenzweig, S. & Koht, A. G. (1933). The experience of duration as affected by need-tension. *Journal of Experimental Psychology, 16*, 745–774.

Rosenzweig, S. & Mason, G. (1934). An experimental study of memory in relation to the theory of repression. *British Journal of Psychology, 24*, 247–265.

Rucci, A. J., & Tweney, R. D. (1980). Analysis of variance and the "second discipline" of scientific psychology: A historical account. *Psychological Bulletin, 87*, 166–184.

Samelson, F. (1992). The APA between the World Wars: 1918–1941. In R. B. Evans, V. S. Sexton, & T. C. Cadwallader (Eds.), *The American Psychological Association: A historical perspective* (pp. 119–148). Washington, DC: American Psychological Association.

Skinner, B. F. (1931). The concept of the reflex in the description of behavior. *Journal of General Psychology, 5*, 427–457.

Skinner, B. F. (1938). *The behavior of organisms*. New York: Appleton-Century-Crofts.

Skinner, B. F. (1953). *Science and human behavior*. New York: The Free Press.

Skinner, B. F. (1998). The experimental analysis of operant behavior: A history. In R. W. Rieber & K. Salzinger (Eds.), *Psychology, Theoretical/historical perspectives*, 2nd ed. (pp. 289–298). Washington, DC: American Psychological Association.

Smith, L. (1986). Behaviorism and logical positivism: A reassessment of the alliance. Stanford, CA: Standford University Press.

Tolman, E. C. (1938). Determiners of behavior at a choice point. *Psychological Review, 45*, 1–41.

Tolman, E. C. (1966). Psychology vs. immediate experience. Originally published in *Philosophy of Science*, July, 1935. Reprinted in E. C. Tolman, *Behavior and psychological man: Essays in motivation and learning*, (pp. 94–114). Berkeley, CA: University of California Press.

Tolman, E. C. (1967). *Purposive behavior in animals and men.* New York: Appleton Century Crofts. (Original work published 1932).

Tolman, E. C. (1952). Edward Chace Tolman. In H. S. Langfeld, E. G. Boring, H. Werner, & R. M. Yerkes, Eds. *A history of psychology in autobiography, Volume IV* (pp. 323–340). Worcester, MA: Clark University Press.

Underwood, B. J. (1949). *Experimental psychology*. New York: Appleton-Century-Crofts.

Van Hoorn, W. (1983). Psychology and the reign of technology. In S. Bem, H Rappard, & W. van Hoorn, (Eds.), *Studies in the history of psychology and the social sciences*. no. 1 (pp. 105–118). Leiden: Psychologisch Instituut van de Rijksunivesiteit Leiden.

VandenBos, G. R. (1992). The APA knowledge dissemination program: An overview of 100 years. In R. B. Evans, V. S. Sexton, & T. C. Cadwallader (Eds.), *The American Psychological Association: A historical perspective* (pp. 347–390). Washington, DC: American Psychological Association.

Weiten, W. & Wight, R. D. (1992). Portraits of a discipline: An examination of introductory textbooks in America. In A. E. Puente, J. R. Matthews, & C. L. Brewer (Eds.), *Teaching psychology in America: A history* (pp. 453–502). Washington, D. C.: American Psychological Association.

Wilson, M. O. & Hodges, J. H. (1932). Predicting success in the engineering college. *Journal of Applied Psychology, 16*, 343–357.

Winston, A. S. (1988). "Cause" and "experiment" in introductory psychology: An analysis of R. S. Woodworth's textbooks. *Teaching of Psychology, 15*, 79–83.

Winston, A. S. (1990). Robert Sessions Woodworth and the "Columbia Bible": How the psychological experiment was redefined. *American Journal of Psychology, 103*, 391–401.

Winston, A. S. (1998). "The defects of his race . . . ": E.G. Boring and antisemitism in American psychology, 1923–1953. *History of Psychology, 1*, 27–51.

Winston, A. S. (2001). Cause into function: Ernst Mach and the reconstruction of explanation in psychology. In Green, C. D., Shore, M. & Teo, T. (Eds.), *The transformation of psychology: Influences of 19th-century philosophy, technology, and natural science* (pp. 107–131). Washington, DC: American Psychological Association.

Winston, A. S., & Blais, D. J. (1996). What counts as an experiment: A transdisciplinary analysis of textbooks, 1930–1970. *American Journal of Psychology, 109*, 599–616.

Woodworth, R. S. (1934). *Psychology* (3rd ed.). New York: Henry Holt.

Woodworth, R. S. (1938). *Experimental psychology*. New York: Henry Holt.

Young, J. R. (1833). *The elements of the differential calculus*. London: John Souter.

PARIS, LEIPZIG, DANZIGER, AND BEYOND[1]

Pieter J. van Strien

INTRODUCTION

Kurt Danziger's analysis of the history of psychological research methodology, in his *Constructing the Subject* (1990)[2], is perhaps the most valuable, and certainly the most cited of his many contributions to the history of psychology. In this book Danziger shows convincingly that the presently prevailing research design of statistically comparing the outcomes of experimental and control groups—Danziger calls it the *(neo-)Galtonian model*—is only a latecomer. The pioneers of psychology used quite other methods to explore the "laws of consciousness". Danziger designates the most prominent early research models as the *Leipzig model* and the *Paris model*. He shows that it was not only new scientific insights, but social forces as well that led to the gradual replacement of the Leipzig model, as the leading research paradigm, by the neo-Galtonian model in the course of the first half of the 20th century.

Danziger's account of the social dynamics of psychological methodology not only has opened a new chapter in the historiography of psychology, but also offers a challenge to historians of psychology to critically examine his conclusions, and further explore developments beyond the reach of his own investigation. In this chapter I shall try to take up this challenge with an eye out for the relationship between subject matter and method. It will appear that the "natural science model", of which both the Leipzig model and the Paris model were offshoots, has persisted in various forms up to the present time, and also that new variants of the neo-Galtonian model have developed in the course of time. Often the new variants

differ to such an extent from the original model that another name is in place. In line with Danziger's contextual approach, I shall also take account of the external "market forces" that are involved.

LEIPZIG AND BEFORE

19TH CENTURY INVESTIGATIVE PRACTICE

Danziger traces the development of his research models back to its 19th century roots. Both the Leipzig model and the Paris model appear to have their origin in mid-19th century physiological and medical investigative practice as ushered in by the French physiologists François Magendie and his pupil Claude Bernard, and by pupils of the German physiologist Johannes Müller. Instead of only describing what they found, they started to study the organism by systematically intervening into the functions of various organs. The (human) body was approached here in the same way as other physical objects, and subjected to invasive probing. A well-chosen case or a crucial intervention sufficed here for establishing a new scientific insight or refuting a rival theory. Others—we could add—mapped the functions of the brain by systematically destroying or extirpating parts of it in animals and noting the effects on behavior. Paul Broca's autopsy of just one patient with a speech defect in 1861 served to convince the scientific world of the localization of the speech center at the base of the third frontal convolution of the left cerebral hemisphere.

Typical of all these investigations is that a single observation or experiment sufficed to demonstrate a particular effect: the action of a poison or a drug, or a deficiency resulting from extirpation of a part of the brain. Replications served only to corroborate or to refine the findings and to remove remaining doubts. This is similar to the practices of the natural sciences. In chemistry, for example, a small quantity of some physical substance suffices to establish its chemical properties, as the object of investigation serves as an arbitrary exemplar of nature in general. Henceforth I shall denote this practice as the *general natural science model.*

When physicians and psychiatrists in Paris started to explore the secrets and aberrations of the human mind, they used this general natural science model for the investigation of human subjects. The assumptions that guided their investigation remained the same, and one or a few subjects were sufficient for explaining the phenomena under study. The new element that was introduced in the *Paris model* (Charcot) was the use of (living) human subjects as subjects (the clinical study of hypnotic phenomena). At this point the *role relationship* between experimenter and subject becomes a crucial factor, and it is here that the roads from "Paris" and "Leipzig" divide. In the Paris model, as in all experiments mentioned so

far, the role relationship was typically asymmetrical. The situation was defined in medical terms. In this respect the Paris model was molded on the basic assumptions that guided physiological and clinical research for most of the 19th century (*see* Bernard, 1865/1957). The medical context implied yet another difference: Whereas the Leipzig model was meant "to display universal processes that characterized all normal minds", the Paris model, as a typical clinical model, meant to "display the effects of an abnormal condition" (Danziger, p. 54). As an example Danziger refers to Charcot's demonstrations of hypnosis and grand hysteria.

Danziger does not follow up the further development of the Paris model, but spends only a few pages on it. He points to Binet's experiments on infants (in fact his daughters Madeleine and Alice; *see* Pollack & Brenner, 1969) as an extension of the Paris model, but does not mention Binet's experiments on great calculators and blindfold chess-masters (Binet & Henneguy, 1894). In both cases the social structure of the experimental situation was the same as with hypnotic subjects, but the context was no longer medical. In a later section I shall give some 20th century examples of non-clinical research in which elements of the Paris model can be traced.

Josef Breuer's and Sigmund Freud's clinical case studies of hysterical patients appear also to have be guided by the assumptions of the Paris model. The same is true for the many clinical case studies spawned by the various schools in psychotherapy that emerged in the 20th century. There was, however, a gradual change of emphasis. The "case" originally served primarily as an exemplar of a typical clinical picture, an abnormal variant of human nature, such as hysteria, schizophrenia and dementia. With time, it acquired more and more an interest in itself, the chief value of which was to demonstrate an author's therapeutic method: the general was exchanged for the typical, and structure for process.

In the *Leipzig model* (Wundt) the role of single subjects as data sources is preserved, but a new element is introduced: the *interchangeability* of the roles of experimenter and experimental subject. Prior to Wundt the symmetry between experimenter and subject can already be found in Franciscus Donders' epoch making reaction-time experiments (Donders, 1868/1969). He and his pupil de Jaager took turns as experimenter and subject. Nevertheless it is appropriate to attach Wundt's name to the new investigative style, because it was in his school that it became a new research tradition.

At first sight this arrangement seems to be merely a didactical device, ensuing from the principle of the *Einheit der Forschung und der Lehre*, and meant to acquaint students with both the role of experimenter and of subject and—once psychology had begun to emulate the natural sciences—to share the tedium of laboratory work. In fact there was a much more fundamental difference. In the Paris model the data consisted of responses and symptoms that were accessible to an outward observer. In the Leipzig model they consisted of the contents of

the subject's own consciousness—phenomena that were accessible only to the subject's own introspection (in the sense of *innere Wahrnehmung*). The best way to do justice to this changed role was to make the subject a *co-researcher*. Making the research relationship symmetrical and the roles interchangeable was the natural next step. In view of the perils of a reliable inner perception, the role of subject (*Beobachter, observer*) even became the most important of both roles. To give a reliable account of one's perceptions required much experience. In the report of his investigations in the *Zeitschrift für Psychologie* the Dutch pioneer of psychology Gerard Heymans legitimated the use of his wife as his principal subject by assuring that she was "*eine sehr geübte Beobachterin*" (Heymans, 1887, p. 132). Elsewhere he defended working with a single subject with the argument that in an absolute sense the data may show personal differences, but that nonetheless the general laws of consciousness will appear in the *relationship* between the data (Heymans, 1896).

As an indication of the importance attached to the role of subject Danziger points to the convention of recording the names or initials of the subjects in experimental reports, as a kind of guarantee of trustworthiness. Not seldom the "great man" put himself in the role of subject. Wundt sometimes fulfilled this role in the experiments of his pupils, particularly in the early ones. In the Göttingen laboratory, the second in rank after the Leipzig laboratory, the *Ordinarius* G.E. Müller also volunteered in taking this role (Katz, 1934). Boring (1953) relates in his *History of introspection* that in Wundt's laboratory no observer who had performed less than 10.000 introspectively controlled reactions was deemed suitable to provide data for published research.[3] The role of experimenter was less fundamental, and consisted solely in administering the stimuli to which the subject had to react and in noting the responses.

The division of roles that became standard in the Leipzig laboratory also served to shield the subject's introspection from distorting influences. Of course, there were situations in which the subject was able to administer the stimuli himself. Fechner's psychophysical experiments are an early example, and Ebbinghaus' research on memory another. But as the task of the *observer* grew more complex, it became increasingly difficult for individuals to experiment on themselves without assistance. In Danziger's words: "The task of simultaneously manipulating the apparatus and playing the role of the possessor of a shielded private consciousness whose precise responses were the object of investigation was not easy, and was sometimes downright impossible." Precondition for a reliable response was to keep the responding individual "in ignorance of the precise short-term variations in the stimulus conditions to which he was to respond." (p. 30)

In the systematic introspection of Wundt's pupils the subject's role became even more important. We find this reflected in Titchener's *Manual of Laboratory Practice* where he admonishes his students: "Introspection is never easy; it becomes doubly difficult when one knows that E desires one to reach a predetermined result. Many experiments have been spoiled by some suggestion from E,

and an answering complaisance on the part of *O*" (Titchener, 1906, p. xvii). This reads like a foreshadowing of Orne's (1962) *demand characteristics*. This meant that a large part of the responsibility for the reliability of the results rested on the shoulders of the experimenter. The ideal of mutual cooperation between experimenter and subject in their search for the secrets of the psyche is phrased in a striking way in the following quotation from Oswald Külpe:

> Thus . . . the typical human relationship between the experimenter and his subjects is the pivotal point of the functioning of the Psychological Institute. Both are contained in a relationship of trust. The social bonds of mutual consideration and self-efficacy and reciprocal understanding and friendly cooperation form the basis for scientific progress. . . . The psychological experiments foster not only our theoretical insight, but also our human worth. (quoted after Graumann, 1952, translation PvS)

In conclusion we can say that in Wundt's Leipzig laboratory nature became human nature, but that the principle of taking the data of a single subject as the basis for establishing general insights into human nature was retained. The research situation changed insofar the contents of the subject's own consciousness now became the central focus of study—phenomena that were accessible only to his or her own introspection. The subject became a co-researcher, and, consequently, the role relationship symmetrical. For the rest the basic premise of the natural science model was preserved: just as the subject of investigation is conceived there as an arbitrary specimen of the whole class to which it belongs, the human subject is conceived here, in the phrasing of Danziger, as an *exemplar of the generalized human mind*.

OTHER EARLY VARIANTS OF THE GENERAL NATURAL SCIENCE MODEL

Before turning to the new methodological model that became prominent in the beginning of the 20th century, two special forms of investigative practice deserve our attention. The first of them is the *demonstrative experiment*. The experimental setting here serves solely to generate a phenomenon that speaks for itself, and the role of subject (or better of *observer*) is fulfilled by anybody who is willing to attend. Phenomenology and Gestalt psychology have become famous in this context, but the 19th century abounds already of other instances. Boring (1950, p. 602) characterizes it as "the convincing single demonstration of some observed generality", and expands on it as follows:

> Purkinje's watching the colors change at dawn is such an instance. Since phenomenology deals with immediate experience, its conclusions are instantaneous. They emerge at once and need not wait upon the results of calculations derived from measurements. Nor does a phenomenologist use statistics, since a frequency does not occur at a given instant and can not be immediately observed.

Of course one can dispute whether this investigative practice still can be regarded as experimentation or solely consists of the demonstration of particular effects. Albert Michotte, undisputedly the most famous 20th century Belgian psychologist, once remarked to a visitor "Don't take me for a Gestalt psychologist—I do real experiments on subjects!"[4] In fact, the investigative practice that is at stake here is not far removed from the demonstrations of patients as practiced within the French tradition. But we should keep in mind that prior to the demonstration there was the *discovery* of an interesting phenomenon. So I am inclined to regard the demonstrative experiment as a (weak) form of experimentation. Because of the potential interchangeability of the roles of "experimenter" and of "subject" we could speak of a hybrid somewhere between the "Leipzig model" and the "general natural science model".

The *case study* is another instance of an investigative practice rooted in the natural science model—at least in so far as the case serves as a basis for theory building. In their book on *Single case experimental designs* Barlow and Hersen (1984) point to Paul Broca's localization of the speech center as an early example of the case study methodology, that acquired such a prominent place in the social sciences later on. The authors also place Ivan Pavlov's later conditioning experiments on dogs in this single-case category, and perhaps we could also take Wolfgang Köhler's experiments on apes and on his infant daughter as an example. Particularly in sociology the case study has acquired a prominent place, but its significance for psychology should not be underestimated, as, for example, appears from Yin (1989), a book that no one less than Donald Campbell has provided with a foreword. Just as in the experiments mentioned above, the case serves as a basis for general theory building. It is not the place here to further expand on this method. For clarity's sake I only must warn that this use of single cases as a basis for general insights should not be confounded with the single-case N = 1 methodology to which I shall return later.

THE "TRIUMPH OF THE AGGREGATE"

The roots of this new model lay not in the laboratory but in differential psychological practice, as initiated by Francis Galton in his anthropometric studies in the 1880s. It differed in two respects from the Leipzig model. In Leipzig the roles of experimenter and subject were symmetrical and interchangeable, while in the new model the role relationship shows a clear hierarchy. And, still more importantly, single subjects serve no longer as data sources in their own right, but merely as anonymous representatives of some statistical class. To allow generalization subjects have to form a representative *sample* of the category they represent. What previously counted as error variance became now, as William Stern (1900) observed, a matter of interest in itself.

Galton too had his forerunners: Gustav Theodor Fechner for example also used group data. In his famous aesthetic investigations into the golden section he did this, and also in his attempt at polling the preference of visitors for one of both Madonnas of Holbein at an exhibition at the Dresden *Zwinger* in the early 1870s (Fechner, 1876). Woodworth (1950, p. 371) calls Fechner a forerunner of the census method, and Sprung and Sprung (1988) call him a precursor of modern sampling methodology. In his posthumous *Kollektivmaßlehre* (1897) Fechner has further elaborated this methodology. He cites here the famous Belgian statistician Adolphe Quetelet, whose probability theory and ideas on *l'homme moyen*—the average (or better: standard) man—also served as a source of inspiration to Galton. Thus, we can say that Fechner, in fact, belonged to the initiators of two experimental models. But it is true, that the model owes its success to the work of Galton.

With Galton measurement still served primarily to satisfy his inquisitiveness about the distribution of the physical and psychical characteristics among natural and social groups within the population, and subsequently as a basis for promulgating his eugenetic ideas. It was, as Danziger shows convincingly, the interest that educational administrators in America took in the method that gave the impetus to the further elaboration and diffusion of his model. Here the first step was made towards comparing *artificial* "treatment groups" with control groups: the "neo-Galtonian model". In this form the model had an enormous success, also outside the "primary market" of education. Under the Roosevelt administration it became, as Trudy Dehue (2001) has shown, the method *par excellence* of social science policy research. Here it gained the status of a watertight warrant of scientific objectivity and impartiality. In the first half of the 20th century the use of control groups, at first limited almost exclusively to applied research, gradually gained the upper hand in laboratory experimentation as well. The fact that the use of controls and randomization as such was part of experimental methodology already from the 1870s onward (Dehue, 1997, 2000) certainly has facilitated adoption of the control-group model.

Danziger concludes to an "eclipse of the Leipzig model" (p. 64), and a "triumph of the aggregate" (title of chapter 5). This triumph occurred first in the Anglo-American world and eventually conquered also the European continent. Developments in the second half of the 20th century confirm Danziger's findings. In most present-day research hypotheses are tested by examining the outcomes of some experimental or quasi-experimental design.

Particularly striking is the way the *relationship between the experimenter and the subject* further developed after the period covered by Danziger. Where Galton, Cattell, and other early differential psychologists still had a genuine interest in the natural or social group of which the subject was a representative, "neo-Galtonians" were neither interested in subjects nor in groups, but only in the significance of their experimental interventions. Present-day subjects are, to use the words of Klaus Holzkamp (1972), who examined the presuppositions of the classic and

the modern research styles before Danziger, stripped of their individuality and transformed into abstract "norm-subjects". In this situation it is no wonder that psychology became more and more the science of the behavior of undergraduates, paid either in money or in credit points. Subjects cheat, or respond to the demand characteristics of the situation. In our market oriented society the experimenter-subject relationship increasingly reflects, as Argyris (1968) has pointed out, the management-employee relationship in industry.

Outside the laboratory the Galtonian approach also made headway, notably in its original domain: *psychometrics*. In personality psychology there has been a strong current in favor of exempting the individual from quantification, and re-serving the understanding of individuals in their uniqueness for the historian or the clinician. In this view the unique case is the very opposite of both the exemplary case of the Paris model and the Galtonian aggregate. Drawing on the German philosopher Wilhelm Windelband, Gordon Allport speaks here of the *idiographic* stance, as opposed to standard *nomological* science. The latter is not able to pen-etrate to the core of someone's personality: *societia non est individuorum* (Allport, 1937, p. 3). The individual case is no longer conceived here as an exemplar of a general syndrome but as a matter of interest in its own right. In this sense the case study method became the favorite approach in clinical psychology in the first half of the 20th century (Bolgar, 1965).

Yet the clinical study of the individual case eventually became also a matter of Galtonian measurement. In modern psychometrics the subject is conceived simply as the "point of intersection of a number of quantitative variables" (Eysenck, 1952, p. 18). In reply to Allport, Eysenck, after having acknowledged that Professor Windelband is absolutely unique, adds: "So is my old shoe." When practitioners make predictions about the future behavior of their clients, he argues, they do so by applying the general laws of behavior to the specific case—laws that are based on aggregate knowledge. *Factor analysis* developed as another branch of the Galton-tree.

Galtonian thinking has even penetrated into the *intra*subjective statistical study of individuals. An early example was William Stephenson (1935; 1953) who applied factor analysis in the study of dimensions of individual persons, the so-called *Q-technique*. Baldwin (1942) made a "personal structure analysis" of one woman on the basis of a statistic analysis of her personal letters. Osgood and Luria (1954) analyzed a case of multiple personality with the help of the *semantic differential* ("The three faces of Eve"). Monte Shapiro, Eysenck's colleague at the London Maudsley Clinic, started a program directed at the "experimental study of single cases" (Shapiro, 1951; 1957). The subject's behavior was seen here not as a sample of similar behavior in a wider population, but as a sample of his or her own behavior in similar situations. By studying this behavior *individual laws* were formulated from which the subject's behavior in future situations was predicted. The findings were subsequently used as a basis for therapeutic interventions (see

Davidson & Costello, 1969, for examples of this N = 1 approach). Similar attempts were made by the Dutch clinical psychologist Johan Barendregt in the psychiatric clinic of the Wilhelmina Hospital of the University of Amsterdam in the 1960s (see Dehue, 1995, Ch. 5). Measurement refers here no longer to the statistical universe of human behavior in general but—to use a term coined by Saul Rosenzweig (1951)—to the *idioversum* of one person. In the same vein the Dutch methodologist Adriaan de Groot (1969) speaks of the "total field of the subject's behavior" within which lawful relationships are formulated on the basis of behavior samples.

Whereas the first N = 1 studies still suffered from growing pains, the experimental study of clinical cases increasingly gathered methodological power, particularly as non-random quasi-experimental designs became available (Campbell & Stanley, 1963; Cook & Campbell, 1979). Examples of sophisticated research in this line can be found in Chassan (1979), Barlow and Hersen (1884) and Kazdin (1982; 1992). Time-series analysis appears to be a favorite method here.

The inevitable conclusion from our extended review seems to be that the "triumph of the aggregate" has become even more complete in the second half of the twentieth century than Danziger suggests. As stated already, Danziger attributes the radical transformation of research practice to market forces: the young discipline of scientific psychology "had to contend with the divergent demands from an expert and a lay public." Within the first the "Leipzig" laboratory conventions prevailed. The wish to provide potential clients—initially primarily in the educational domain—with useful practical knowledge gave rise to a new Galtonian approach, in which individuals were ordered in terms of their standing in a statistical aggregate. In this quandary an approach in which a treatment or experimental group and a control group were compared offered itself as a compromise that enabled the discipline "to have it both ways" (Danziger, Ch. 5). This neo-Galtonian model became the new standard. Once it had conquered scientific psychology, the model was transferred back to differential practice, even, as we just saw, at the level of the single case.

LEIPZIG AND BEYOND

Nevertheless, this "triumph of the aggregate" does not necessary imply a *complete* eclipse of the older models. If it is true that the forces of the markets of educational and social administration had such a great influence on psychological methodology then we also must allow, on the one hand, for domains within the discipline where these forces were less influential, on the other hand, for new market forces that lead to new investigative models, or at least modifications of existing models. An exhaustive survey of these possibilities is, of course, not feasible within the scope of this chapter. I confine myself to the further development of "non-Galtonian" models beyond the period to which Danziger's study pertains.

Psychophysics and Psychophysiology

Kurt Danziger himself recognizes one field in which the Leipzig model is still pertinent to the object of investigation. On p. 70 of his book he writes: "In such areas as the psychology of sensation results from even a single subject can be claimed to have general significance, because of the presumed similarity of the underlying physiology in all human individuals." The "hardware" of our mental apparatus, or the "architecture" of our system, as present day experimental psychologists call it, is at stake here, and its characteristics can be ascertained from any healthy organism.

In fact this was the domain in which I first was alerted to the persistence of the Leipzig research style in contemporary experimental research (van Strien, 1993b, 1995, 1996). In a survey of doctoral dissertations at various Dutch universities I found that, indeed, in line with the international trend, the use of the Leipzig model did decrease after the Second World War. However, contrary to my expectations, about 50% of the key-figures of post-war experimental psychology in the Netherlands—all future professors—still followed it in their doctoral dissertation and in subsequent experimental studies. Among them were Willem Levelt, later to become director of the internationally renowned *Max Planck Institute of Psycholinguistics* in Nijmegen, and John Michon, who later became president of the Dutch *Psychonomic Society*, and first editor of an international *Handbook of Psychonomics.*

Levelt used only a few subjects in his dissertation *On binocular rivalry* (1965). In line with the Wundtian tradition he called them *observers* and identified them with their initials, one of them (W.L) being Levelt himself. In some of his investigations he used only two or three subjects. Michon stuck in his dissertation on *Timing in temporal tracking* (1967) still closer to the Wundtian tradition. His six subjects were, he assured the reader, well-trained; he identified them by initials and Michon himself (M) was one of them. In some experiments so-called "naive" subjects were used, but he threw out—just as Titchener did!—the results of those who appeared to be unreliable.

One explanation for the so much higher percentage in the Netherlands—even in the 1960s—could be an enduring European orientation in the Dutch laboratories. It is true, indeed, that after World War II it took a considerable time before Dutch psychologists were won over to the Anglo-American perspective (van Strien, 1997). Is the saying, ascribed to Heinrich Heine, right, then, that Holland is the best place to go at the Day of the Last Judgement, because everything happens there thirty years later? For the young researchers just mentioned this time-lag does not hold, however. And, what is more, depending on the subject matter, the (neo-) Galtonian approach was used by Dutch researchers just as often. This led me to investigate in how far the Leipzig practice of using one or only a few subjects as the knowledge base was—and perhaps still is—part of the standard practice in certain

quarters of the scientific community, particularly in the domain of psychophysics and psychophysiology.

An inspection of representative publications showed that in nearly all investigations in this field the number of subjects was much smaller (less than ten or even five) than in studies of higher levels of human or animal behavior. Often they were identified by numbers or by their initials. Even where the subjects were anonymous and were treated as a statistical aggregate their number was usually small. In a relatively recent volume of the interdisciplinary journal *Vision Research*[5] over 80% of the studies dealing with human subjects followed the "Leipzig" canon. The number of subjects was small (rarely more than five); they usually were called "observers"; and in three-quarters of the cases they were identified by their initials. In the majority of the cases (60%) one or more of the authors served as observers. The subjects often were described as either "well trained" or "naive". The results were nearly always presented in graphs of figures for each separate subject, often accompanied with remarks on the status of his or her vision. We must conclude, that the research style in this field has changed little since the era of Helmholz and Hering, or, to cite a more recent example, since the days that Boring (1943) conducted his experiment on the moon illusion—also mainly based on the data of two long-standing observers!

Striking examples of small N research can also be found in the area of *motor control and skill acquisition*. In an authoritative reader in this field (Stelmach, 1978) the model is applied in seven out of the ten empirical articles. The same holds for applied psychophysical research, such as the *ergonomic* design of displays and research in motor control. Space technology is only one of the many examples. A related area in which evidence usually is derived from only a few subjects or cases is *brain-and-behavior research*: studies of, for instance, aphasia or amnesia in relation to brain injuries or other traumata. Thus, Damasio's (1994) study on emotion, reason and the human brain is based on only a few clinical cases of brain damage.

Of course it would be incorrect to conclude from these examples that the Leipzig model has persisted up to the present day in the form in which it was practiced under Wundt. The use of interchanging experimenters, for instance, has not survived. What really survived was the classical natural science tradition of according general significance to the results from very small numbers of subjects.

Operant Conditioning: Skinner

Another example of research in which this tradition is still alive is operant conditioning in the line of B.F. Skinner.[6] Unlike most behaviorists Skinner held group data in low esteem, and was an ardent advocate of *single case* operant conditioning. Perhaps his approach is closest to Pavlov's conditioning of single dogs. In an address to the Pavlovian Society of America in 1966 he acknowledged the strong bond between himself and Pavlov (*See* Barlow & Hersen, 1984, p. 5).

Just like the 19th century physiologists, Skinner experimented on single subjects. However, where they followed an invasive approach, his method consisted of repeated objective measurement in intact, healthy subjects over a long time under highly controlled conditions. The principles of this approach are laid down in Sidman (1960), a book that, according to Skinner (1983, p. 266), "became a kind of Bible among operant conditioners."

How far Skinner's method deviates from the neo-Galtonian tradition appears from the following quote: "Operant methods make their own use of Grand Numbers: instead of studying a thousand rats for one hour each, the investigator is likely to study one rat for a thousand hours" (Skinner, 1966, p. 21). In his autobiography (1983, p. 123) he relates how, after having reported at an APA-conference in the 1950s on the results of an experiment on one rat, he provoked his audience by continuing: "in deference of the standards of this Association I will now report on the other rat". On the other hand, this quote gives evidence of the degree to which the neo-Galtonian model already had become standard practice in American psychology at that time.

The reservations journal editors had toward small-N studies were a major reason for the founding of the *Journal of the Experimental Analysis of Behavior* (JEAB) in 1958, edited by the Skinnerian *Society for the Experimental Analysis of Behavior*. In 1968 the Society started publishing a sister journal, the *Journal of Applied Behavior Analysis* (JABA). Both journals carry hosts of single-case and small-N studies. David Krantz (1971) compared the design of animal experiments in the JEAB and the JABA with that in the *Journal of Comparative and Physiological Psychology*. He found that in 1969 87.5% of the articles in both Skinnerian journals followed an "*intra*-individual" design, against 88.5% cases of an "*inter*-individual" design in the JCPP. Quite appropriately Krantz speaks of "two separate worlds". Many examples of small-N research can also be found in the Skinnerian reader *Operant Behavior* (Honig, 1966).

Würzburg and the Cognitive Revolution

I now come to a domain of research that Danziger did not recognize, probably because it came to prominence only in the second half of the 20th century, and thus lies beyond the scope of his examination of investigative practices. It is the field of the cognitive psychology of thinking, and the rapidly developing study of Artificial Intelligence, including the building of expert systems that issued from it.

The cognitive psychology of thinking has its roots in the *systematic introspection* of the Würzburg School around Oswald Külpe, an approach in which the introspective stance of the Leipzig model is further extended. In his book (pp. 42–44) Danziger characterizes the systematic experimental introspection of Külpe, Titchener, and other Wundt-pupils as "a dead end" and "a rather odd episode", and its fate as "a debacle". It is certainly true, that the debate between the

principal protagonists about the proper conduct of introspection ended in a blind alley in the first decades of the century. However, this should not make us forget that a specific variant of introspection or retrospection, to wit *thinking aloud while fulfilling a particular task*, has opened up a very prolific line of investigation. The most important representatives of this line of investigation were Külpe's pupil Otto Selz (1913, 1922), Gestalt psychologist Karl Duncker (1926, 1935) and the Swiss psychologist Edouard Claparède (1917, 1932).

The *Selbstbeobachtung* they asked of their subjects differed fundamentally from the simple inner perception required by Wundt, and was, for that very reason, rejected by him. But fundamental characteristics of the Leipzig model, like the reporting of individual data and interchangeability of experimenter and subject, were preserved. One could even say that these characteristics were amplified in the Würzburg-style experiments: being a well-trained subject became a still more crucial factor here.

The Nazi regime has nearly eradicated this line of research (both Duncker, an open anti-fascist, and Selz, a Jew, were removed from their posts, and Selz even met his end in a concentration camp), but after some time a revival occurred. One of the links in this development has been the study on *Thought and Choice in Chess* by de Groot (1946/1965). Inspired by the work on productive thinking of Selz, who came as a refugee to Amsterdam shortly before the outbreak of the Second World War, de Groot used the thinking-aloud method to trace the thought processes of grandmasters and other chess-players. They were identified by their initials—a typical feature of the Leipzig-Würzburg approach. His example was followed by the Dutch psychologist Jongman (1968) and the Swiss psychologist Gobet (de Groot & Gobet, 1996). The thinking aloud method has been also applied to the thinking of neurologists by Snoek (1989), and by Hamel (1990) to the thinking of architects.

Drawing on de Groot, but also on Selz and Duncker and on Bartlett's (1958) study of thinking, Allen Newell and Herbert Simon (1972) used the thinking-aloud protocols of only a few experts as a basis for the construction of mathematical models of problem solving and chess-playing. Subsequently Newell, Shaw and Simon (1958, 1963) implemented these models in computer programs. Simon's pupil Ericsson has developed the method of protocol analysis further (Ericsson & Simon, 1984). In the last decades *Artificial Intelligence* (AI) experienced a veritable boom in the construction of expert systems, instantiated in computer programs. They were based on the problem solving procedures of top experts, that were traced down by means of the thinking-aloud procedure (e.g. Kidd, 1978; McGraw & Harbinson-Briggs, 1989; Ericsson & Smith, 1991). In these recent advancements the goal is no longer the development of psychological theory, but of *technology*: knowledge engineering (Feigenbaum, 1984).

In these studies the relation between experimenter and subject is elevated to the supreme level of exchange between experts—knowledge experts on the one side and task experts on the other side—exploring together the possible reaches of

human cognition and performance. Although as experts they are of equal standing, the symmetry and interchangeability, which were typical of the classic Leipzig experiment, have disappeared here. In this sense, elements of the Paris model have crept in, not so much in its original clinical use of demonstrating hysterical patients and hypnotic phenomena, but in the form in which Binet used it in his investigations into the characteristics of singularly gifted subjects, such as great calculators and blindfold chess-masters. The continuing influence of the Paris model can also be found in studies of creative individuals, such as Bahle's (1936) study of composers.

NAMING THE MODEL

The references made to *elements* of the Leipzig and Paris models that were preserved, imply that it would not be appropriate still to maintain the original nomenclature. How, then, should we name the various modalities under review? I would suggest only designating a model by the name of a special geographic site or historical person if it is a matter of a *school* within which the model originated and from where it further was disseminated. This was clearly the case with the Paris of Charcot and the Leipzig of Wundt. So these designations conform to our criterion. Würzburg qualifies for a position as a model of its own: the way introspection was used by Külpe and his pupils deviates to such an extent from the way envisaged by Wundt, that it seems better to speak here of an separate school. Titchener's introspection deviated also from Wundt's, but one can hardly say that in this respect he established a school. In this sense he represented a "dead end", indeed. In the later cognitive *thinking aloud studies*, cited in section 4, the focus shifts from the normal to the exceptional: special talents or competencies. In this sense elements of the Paris model creep in. In the absence of a specific geographic origin of this new paradigm I propose the term *expert model*. Skinner at first sight seems to be a case in itself, but at a closer look his approach is not more than the resumption of the classical natural science model.

In the case of the Galtonian model Danziger uses not the birthplace but the founding father of the new model. This is disputable, because Galton, just as his predecessor Fechner, certainly was a great source of inspiration—to Karl Pearson and James McKeen Cattell above all—but hardly can be called the founder of a *school*. Danziger acknowledges this himself in the first article in which he proposes his three models. He identifies Clark University as the major center where (under Stanley Hall) the new research practice was first systematically employed and, consequently, speaks of the *Clark model* (Danziger, 1985, p. 137). But in his next publication about research practice (Danziger, 1987) he abandons *the Clark model* and ends up with "Galtonian", after toying with the "Galton-Pearson model". In this situation I personally prefer to part with both geography and paternity

and—as I did already in proposing the term "expert model"—to opt for a name in which the quintessence of the approach is expressed, namely *Differential model.* The Galtonian approach of the single case, as discussed in section 3, already has assumed an appropriate name: the *N = 1 model.*

The neo-Galtonian introduction of artificial groups represents such a fundamentally new step in the logic of experimentation, the importance of which—as Danziger (p. 85) emphasizes—is "difficult to overemphasize", that a new designation is in place. I propose to speak here of *Control group model* in contradistinction of the original Differential model.[7]

However, the real divide lies not between "Leipzig" and "Galton" but between the classical natural science tradition that informed both "Paris" and "Leipzig" and its derivatives on the one hand, and the stochastic way of thinking that came up at the turn of the century on the other hand. This is the pivotal difference in "constructing the subject" around which Danziger's book turns. The fact that this transformation appears to be less complete than Danziger's survey of developments in the first half of the twentieth century suggests, confronts us again with the reason of the persistence of basic elements of the natural science tradition.

METHODOLOGY IN ITS CONTEXT

THE SOCIAL CONTEXT OF INVESTIGATIVE PRACTICE

Danziger conceives of science as a social activity, in line with modern sociology of scientific knowledge. Researchers not only have to appeal to their own research community and representatives of related disciplines for acceptance of their results, but also have to deal with an external environment that furnishes (or withholds) financial support and provides a market for knowledge products.[8] In Danziger's view the methodological shift that was discussed above is the outcome of the contest between these two opposing forces: On the one hand the methods that had brought such impressive successes in the natural sciences were seen by the aspiring discipline as the royal road to discover the *laws* of consciousness, and thus to gain scientific respectability. On the other hand, the external market—the educational market to begin with—soon began to ask for useful products. It was the "pull" from this market that finally led to the adoption of Galtonian differential thinking, first in applied psychology, then also in basic research, in the form of control group methodology. The fact, we could add, that the use of controls and randomization as a methodological precaution had a respectable tradition in experimental psychology certainly has favored the reception of the new approach within the scientific community.

However, accepting the control-group model as a valid tool for particular research problems is not the same as adopting it yourself. Psychophysics

psychophysiology, and the other domains examined in our section 4 had good reasons to adhere to the methods of the "harder" natural sciences, because this was the accepted style in the professional environment in which they operated. My analysis of papers in the journal *Vision Research*, showed that many of the authors were employed in institutes with an interdisciplinary staff. Psychologists in this field don't see themselves as social scientists, but as representatives of the natural sciences. Their publication channels are not controlled by social scientists, but by natural scientists, for whom experiments on single subjects are current practice, because of the invariance of the physical substrate. And where they did operate in a social science environment, as was the case with Skinner and his group, they could resort to entrenching themselves in a sub-community, with its own journals and its own external contacts.

The external market for the products of psychophysical and psycho-physiological research was quite different from the social science market in which the differential approach developed. From World War II onwards, studies on vision and motor control have proved to be of great practical interest to the military and to industry (in such things as navigation, design of displays, handles and control panels). Present-day research institutes in this field have close affiliations with the defense system, industry, or both. Machine recognition of speech and handwriting belong to the more recent examples of applied research in this field. To be an attractive partner in this market, psychologists do better to affiliate with the "hard-nosed" natural scientists, and to follow their methods and manners, and not those of the "softer" social scientists. This was another reason for adhering to the "good old" natural science model.

In the cognitive study of problem solving and artificial intelligence similar contextual factors apply. Research is conducted in an interdisciplinary arena with a strong natural science identity, and published in journals in which the methodological conventions of other psychologists play only a secondary role. In the U.S.A., the Netherlands and some other countries psychologists belonging to this category even have formed a *Psychonomic Society* of their own, outside the professional association of psychologists. As to the external market, strong technical and economical forces appear to play a part here as well—particularly in the applied branches of the field. The prevailing practical interest here is in making a specific *expert performance* marketable. This has led to the new variant of the natural science model for which I proposed the name "expert model".

The "Probabilistic Revolution"

If the natural science model still holds in these prestigious sub-fields of psychology, why is it that "Galtonian" thinking could attain such a strong position? Would the sheer pull from the market for applied research be sufficient to lead to the "triumph of the aggregate?" To my mind the fundamental methodological turn

that took place in the first half of the twentieth century could come about only thanks to an additional factor. This was the "probabilistic revolution" in science that occurred in the same period, and that radically changed the intellectual climate in Western thinking. This revolution has its roots in attempts at "taming chance" of 17th and 18th century gambling aristocrats, calculating merchants, entrepreneurs and rulers (Hacking, 1975), and gradually spread from commerce, insurance and governmental sadministration to science and everyday practice. Chance became a leading principle in scientific research and in rational decision making. Statistics became a *Lebensgefühl*, and a way of life (Gigerenzer et al., 1989, p. 289).

Studies into the impact of the probabilistic revolution on the various sciences are of recent origin (Gigerenzer et al., 1989; Krüger, Daston & Heidelberger, 1987, Krüger; Gigerenzer & Morgan, 1987). The most conspicuous implication is, of course, the subversion of the deterministic worldview of the classical natural sciences. I shall pursue this aspect only briefly, and concentrate on the epistemological aspect. A full appreciation of the implications of probabilistic thinking for the "construction of the subject" would require a chapter of it own.

The natural science approach that informed both the Paris and Leipzig models and their derivatives was characterized by a Laplacian mechanistic, deterministic worldview. Typical in this respect is the assertion of Claude Bernard (1865/1957, p. 136) that ". . . scientific law can be based only on certainty, on absolute determinism, not on probability." In this worldview variance in scientific observations was ascribed to the imperfection of our registration of reality, not to the haphazardness of nature. In spite of their fascination with variance, nineteenth century authors like Quetelet and Galton were still very much impressed with the order in nature (Porter, 1983, p. 36). For them statistics was an instrument for making this order visible.[9] This orderly view on nature had gradually to give way to a probabilistic worldview. Within the natural sciences evolutionary biology and quantum physics led the way in accepting it, whereas research in physiology was much less affected.

As we saw, Leipzig laboratory research was inspired by the natural science tradition. In this line, variability was considered as error around a true value that characterizes a natural process. Probabilistic thinking was tolerated solely as an expression of the experimenter's ignorance. In the control-group model statistical probabilities acquired a much more fundamental role. Though probabilistic thinking here too, as Gigerenzer (1987) notes, was enlisted in the service of determinism, the essential tenet of determinism: establishing true causal connections, was sacrificed here. Causality in nature was assumed, but not traced down. Metaphysical determinism was exchanged for pragmatic determinism, and certainty for probability: a methodological probabilism. Probabilistic statistics, based on sampling and randomization, became the standard tool of inductive inference – quite a different form of orderliness than the order presupposed in the natural science model. This transition ran parallel to the transition from essentialism to pragmatism in applied psychology which I have described elsewhere (van Strien, 1998).

In the second half of the 20th century statistical inference, first only a tool, became a model of the human mind: the theory of the mind as "an intuitive statistician" (viz. Gigerenzer & Murray, 1987). A discussion of the implications of this extended revolution for cognitive psychology lies outside the scope of this chapter. In the present context it may suffice to conclude, that the epistemological "*re*-construction of the subject" described by Danziger could only become so profound thanks to the probabilistic revolution.

CONCLUSION

An examination of investigative practices of psychologists beyond the period reviewed by Kurt Danziger showed that the research style of basing inferences on the experimental data of one subject or just a few subjects—first initiated in Leipzig—continued to play a vital role in several domains. This was not only the case in psychophysical and psycho-physiological research—the areas to which Danziger drew attention himself—but also in the realm of operant conditioning and in some parts of cognitive psychology. In the study of artificial intelligence and expert performance it even witnessed a revival in the form of what I called the expert model. Both Danziger's Leipzig model and his Paris model appeared to be variants of an encompassing *natural science model* that gained prominence in the course of the 19th century. The most fundamental methodological divide appeared to lie not between the Leipzig model and the Galtonian differential model, but between the classical natural science model and the stochastic approach that, as a consequence of the probabilistic revolution, increasingly gained ground in the twentieth century. This led me to a proposal to re-label the current research models. These qualifications, however, do not detract anything from Danziger's explication of the radical transformation in "the construction of the subject" that took place in the course of the past century.

A contribution of equal rank has been the way he has placed this transformation in its social context. Of particular interest for the historian of science is the insight that the methodological shift that took place was not solely due to new scientific insights, but also to developments in the market of psychological services in which psychologists operated. Psychological practice set the tune, and the laboratory followed. In the survival of small-N research in the second half of the 20th century similar market forces appear to have played a significant role.

NOTES

a previous version, and to Adrian Brock and Johann Louw for their encouragement and painstaking editorial work.

[2] When not stated otherwise, the Danziger citations hereafter refer to this book.

[3] Most probably Boring's statement is based on Leipzig laboratory lore, cited by Titchener—himself a pupil of Wundt—to impress the importance of the role of subject upon his disciples.

[4] This visitor was the Dutch psychologist Willem Levelt (personal communication). It should be noted, though, that Gestalt psychologists did not confine themselves to demonstrative experiments, but also applied the traditional Leipzig research style (Ash. 1995, 220–24).

[5] Volume 25, 1985. Because of the bulk of the volume (more than 2000 pages!) I analyzed only the even issues. Though the professional affiliation of the authors could not be ascertained in all cases, it can safely be assumed that at least half of them were working in a psychology department.

[6] Although he does not mention Skinner's name, Danziger appears to be aware of the exceptional position of the operant psychologists, but disposes of this anomaly in only a few words (p. 154).

[7] In introducing these designations, I retract my proposal in my Passau paper (van Strien, 1996) to use the term *competence model* for both the original "Leipzig model" and its later derivatives and the term *sample model* for both the "Galtonian model" and the "neo-Galtonian model".

[8] There is a close similarity between the social context as conceived of by Danziger and the *relational field* of investigative practice in my "relational model" (van Strien, 1991, 1993a, 1993b).

[9] This common element should not make us loose sight of the difference between both authors: to the astronomer Quetelet the average was something of an almost Platonic order, and error was error; to Galton the average was subject to continuous (eugenetic) amelioration (Hilts, 1973, Porter, 1986, Gigerenzer et al., 1989).

REFERENCES

Allport, G.W. (1937). *Personality, A psychological interpretation*. New York: Holt.

Argyris, Chr. (1968). Some unintended consequences of rigorous research. *Psychological Bulletin, 70*, 183–197.

Ash, M.G. (1995). *Gestalt psychology in German culture, 1890–1967*. Cambridge, Mass.: Cambridge University Press.

Bahle, J. (1936). *Der musikalische Schaffensprozess; Psychologie der schöpferischen Erlebnis—und Arbeitsformen*. Konstanz: Christiani.

Baldwin, A.L. (1942). Personal structure analysis: A statistical method for investigation of the single personality. *Journal of Abnormal and Social Psychology, 37*, 163–183.

Barlow, D.H. & Hersen, M. (1984). *Single case experimental designs*. New York: Pergamon.

Bartlett, F. (1958). *Thinking, an experimental and social study*. London: Allen & Unwin.

Bernard, C. (1865/1957). *An introduction to the study of experimental medicine* (English translation). New York: Dover.

Binet, A. & Henneguy, L. (1894). *La psychologie des grands calculateurs et joueurs d'échecs*. Paris: Flammarion.

Bolgar, H. (1965). The case study method. In B.B. Wolman (Ed.), *Handbook of clinical psychology* (pp. 28–39). New York: McGraw Hill.

Boring, E.G. (1943). The moon illusion. *American Journal of Physics, 11*, 55–60.

Boring, E.G. (1950). *A history of experimental psychology*. New York: Appleton-Century-Crofts.

Boring, E.G. (1953). A history of introspection. *Psychological Bulletin, 50*, 169–189.

Campbell, D.T. & Stanley, J.C. (1963). *Experimental and quasi-experimental design for research*. Chicago: Rand McNally.

Chassan, J.B. (1979). *Research design in clinical psychology and psychiatry*. New York: Appleton-Century-Crofts.

Claparède, E. (1917). Psychologie de l'intelligence. *Scientia, 22*, 353–368.

Claparède, E. (1932). Genèse de l'hypothèse. *Archives de Psychologie, 24*, 1–155.

Cook, T.D. & Campbell, D.T. (Eds.) (1979). *Quasi-experimentation: Design and analysis issues for field settings.* Chicago: Rand McNally.

Damasio, A.R. (1994). *Descartes' error; Emotion, reason, and the human brain.* New York: Putnam.

Danziger, K. (1985). The origins of the psychological experiment as a social institution. *American Psychologist, 40*, 133–140.

Danziger, K. (1987). Statistical method and the historical development of research practice in American psychology. In L. Krüger et al. (Eds.), *The probabilistic revolution Vol. II* (pp. 35–47). Cambridge, Mass.: MIT Press.

Danziger, K. (1990). Constructing the subject; historical origins of psychological research. New York: Cambridge University Press.

Davidson, P.O. & Costello, C.G. (1969). *N = 1: Experimental studies of single cases.* New York: van Nostrand.

Dehue, T. (1995). *Changing the rules; Psychology in the Netherlands, 1900–1985.* Cambridge: Cambridge University Press.

Dehue, T. (1997). Deception, efficiency, and random groups; Psychology and the gradual origination of the random group design. *Isis, 88*, 653–673.

Dehue, T. (2000). From deception trials to control reagents; The introduction of the control group about a century ago. *American Psychologist, 55*, 264–268.

Dehue, T. (2001). Establishing the experimenting society: The historical origin of social experimentation according to the randomized controlled design. *American Journal of Psychology, 114*, 283–302.

Donders, F.C. (1868/1969). On the speed of mental processes (English translation). *Acta Psychologica, 30*, (Special Issue on Attention and Performance), 412–431.

Duncker, K. (1926). A qualitative (experimental and theoretical) study of productive thinking (solving of comprehensible problems). *Pedagogical Seminary, 33*, 642–708.

Duncker, K. (1935). *Zur Psychologie des produktiven Denkens.* Berlin: Julius Springer.

Ericsson, K.A. & Simon, H.A. (1984). *Protocol analysis; Verbal reports as data.* Cambridge Mass.: MIT Press.

Ericsson, K.A. & Smith, J. (Eds.) (1991). *Toward a general theory of expertise*. Cambridge: Cambridge University Press.

Eysenck, H.J. (1952). *The scientific study of personality*. London: Routledge & Kegan Paul.

Fechner, G.Th. (1876). *Vorschule der Aesthetik*. Leipzig: Breitkopf und Härtel.

Fechner, G.Th. (1897). *Kollektivmaßlehre*. Leipzig: Engelmann.

Feigenbaum, E.A. (1984). Knowledge engineering; the applied side of artificial intelligence. *Annals of the New York Academy of Science, Vol. 426 (Special Issue: Computer Culture)*, 97–107.

Gigerenzer, G. (1987). Survival of the fittest probabilist: Brunswik, Thurstone, and the two disciples of psychology. In: Krüger, L., Gigerenzer, G. & Morgan, M. (Eds.) (1987). *The probabilistic revolution II: Ideas in the sciences* (pp. 49–72). Cambridge, Mass.: MIT Press.

Gigerenzer, G. et al. (1989). *The empire of chance; how probability changed science and everyday life.* Cambridge: Cambridge University Press.

Gigerenzer, G. & Murray, D.J. (1987). *Cognition as intuitive statistics.* Hillsdale NJ: Erlbaum.

Graumann, C. (1952). *Die Kriterien des Einfallserlebnis* (Inauguraldissertation Köln), Ed. 1955.

Groot, A.D. de (1946/1965). *Thought and choice in chess*. (English translation) Den Haag: Mouton.

Groot, A.D. de (1969). *Methodology; Foundations of inference and research in the behavioral sciences*. The Hague: Mouton.

Groot, A.D. de & Gobet, F. (1996). *Perception and memory in chess. Studies in the heuristics of the professional eye*. Assen: Van Gorcum.

Hacking, I. (1975). *The emergence of probability*. Cambridge: Cambridge University Press.

Hamel, R. (1990). *Over het denken van de architect, een cognitief psychologische beschrijving van het ontwerpproces bij architecten*. Amsterdam: AHA-Books.

Heymans, G. (1887). Quantitative Untersuchungen über die Zöllnersche und Loebsche Täuschung. *Zeitschrift für Psychologie und Physiologie der Sinnesorgane, 14*, 101–139. [Also in: *Gesammelte kleinere Schriften II*, pp. 35–71. Den Haag: Nijhoff].

Heymans, G. (1896). Een laboratorium voor experimenteele psychologie. *De Gids, 60*, Dl. II, 73–100.

Hilts, V.L. (1973). Statistics and social science. In R.N. Giere & R.S. Westphal (Eds.), *Foundations of scientific method* (pp. 206–233). Bloomington, Ind.: Indiana University Press.

Holzkamp, K. (1972). Verborgene anthropologische Voraussetzungen der allgemeinen Psychologie. In K. Holzkamp. *Kritische Psychologie* (pp. 35–73). Frankfurt a. M: Fischer.

Honig, W.K. (Ed.) (1966). *Operant behavior: Areas of research and application*. New York: Appleton Century Crofts.

Jongman, R.W. (1968). *Het oog van de meester, een experimenteel-psychologisch onderzoek naar waarnemingsprestaties van schaakmeesters en ongeoefende schakers*. Assen: Van Gorcum.

Katz, D. (1934). Würdigung G.E. Müller. *Acta Psychologica, 1*, 234–240.

Kazdin, A.E. (1982). *Single-case design: Methods for clinical and applied settings*. New York: Oxford University Press.

Kazdin, A.E. (1992). *Research design in clinical psychology*. Needham Heihts, Mass.: Allyn & Bacon.

Kidd, A.L. (Ed.) (1978). *Knowledge acquisition for expert systems*. New York: Plenum.

Krantz, D.L. (1971). The separate worlds of operant and non-operant psychology. *Journal of Applied Behavioral Analysis, 4*, 61–70.

Krüger, L., Daston, L. & Heidelberger, M. (Eds.) (1987). *The probabilistic revolution I: Ideas in history*. Cambridge, Mass.: MIT Press.

Krüger, L., Gigerenzer, G. & Morgan, M. (Eds.) (1987). *The probabilistic revolution II: Ideas in the sciences*. Cambridge, Mass.: MIT Press.

Levelt, J.M. (1965). *On Binocular Rivalry*. Assen: Van Gorcum.

McGraw, K.L. & Harbison-Briggs, K. (1989). *Knowledge acquisition; principles and guidelines*. Englewood Cliffs, N. Jersey: Prentice Hall.

Michon, J.A. (1967). *Timing in temporal tracking*. Assen: Van Gorcum.

Newell, A., Shaw, J.C. & Simon, H.A. (1958). Elements of a theory of human problem solving. *Psychological Review, 65*, 151–166.

Newell, A., Shaw, J.C. & Simon, H.A. (1963). Empirical explorations with the logic theory machine: a case study in heuristics. In: E.A. Feigenbaum & J. Feldman (Eds.), *Computers and Thought* (pp. 109–133). New York: McGraw-Hill.

Newell, A. & Simon, H. A. (1972). *Human problem solving*. Englewood Cliffs, N.J.: Prentice-Hall.

Orne, M.T. (1962). On the social psychology of the psychological experiment: with particular reference to demand characteristics and their implications. *American Psychologist, 17*, 776–783.

Osgood, Ch.E. & Luria, Z. (1954). A blind analysis of a case of multiple personality using the semantic differential. *Journal of Abnormal and Social Psychology, 49*, 579–591.

Pollack, R.H. & Brenner, M.W. (Eds.) (1969). *The experimental psychology of Alfred Binet; Selected papers*. New York: Springer.

Porter, T.M. (1983). Private chaos, public order: The Nineteenth-Century statistical revolution. In M. Heidelberger, L. Krüger & R. Rheinwald (Eds.), *Probability since 1800* (pp. 27–40). Bielefeld: Universität Bielefeld; Wissenschaftsforschung/Science studies, Report 25.

Porter, T.M. (1986). *The rise of statistical thinking 1820–1900*. Princeton, N.J.: Princeton University Press.

Rosenzweig, S. (1951). Idiodynamics in personality theory with special reference to projective methods. *Psychological Review, 38*, 213–223.

Selz, O. (1913). *Ueber die Gesetze des geordneten Denkverlaufs. Eine experimentelle Untersuchung.* Stuttgart: Spemann. (English excerpts in: N.H. Frijda en A.D. de Groot (Eds.) (1981), *Otto Selz; his contribution to psychology*. (pp. 76–146). The Hague: Mouton).

Selz, O. (1922). *Zur Psychologie des produktiven Denkens und des Irrtums.* Bonn: Cohen.

Shapiro, M.B. (1951). An experimental approach to diagnostic psychological testing. *Journal of mental science, 97*, 748–764.

Shapiro, M.B. (1957). Experimental method in the psychological description of the individual psychiatric patient. *International Journal of Social Psychiatry, 3*, 89–102.

Sidman, M. (1960). *Tactics of scientific research; Evaluating experimental data in psychology.* New York: Basic Books.

Skinner, B.F. (1966). Operant behavior. In W.K. Honig (Ed.), *Operant behavior: Areas of research and application* (pp. 12–32). New York: Appleton Century Crofts.

Skinner, B.F. (1983). *A matter of consequences.* (Part three of an autobiography). New York: Knopf.

Snoek, J.W. (1989). *Het denken van de neuroloog.* Dissertation Rijks-Universiteit Groningen.

Sprung, H. & Sprung, L. (1988). Gustav Theodor Fechner als experimenteller Ästhetiker—Zur Entwicklung der Methodologie und Methodik einer Psychophysik höherer kognitiver Prozesse. In J. Brozek & H. Gundlach (Eds.), *G.T. Fechner and Psychology* (pp. 217–227). Passau: Passavia Universitätsverlag.

Stelmach, G.E. (1978). *Motor control: issues and trends.* New York: Academic Press.

Stephenson, W. (1935). Correlating persons instead of tests. *Character and Personality, 6*, 17–24.

Stephenson, W. (1953). *The study of behavior; Q-technique and its methodology.* Chicago: University of Chicago Press.

Stern, W. (1900). *Über Psychologie der individuallen Differenzen; Ideen zu einer differentiellen Psychologie.* Leipzig: Barth.

Strien, P.J. van (1991). Audiences, alliances, and the dynamics of science. Transforming Psychology in the Netherlands II. *History of the Human Sciences, 4*, 351–369.

Strien. P.J. van (1993a). The historical practice of theory construction, In: H.V. Rappard, P.J. van Strien, L.P. Mos & W.J. Baker (Eds.) Theory and History; *Annals of Theoretical Psychology, Vol. VIII* (pp. 149–228). New York: Plenum Press.

Strien. P.J. van (1993b). Nederlandse psychologen en hun publiek; Een contextuele geschiedenis. Assen: Van Gorcum.

Strien, P.J. van (1995). Der Experimentierstil in den niederländischen psychologischen Laboratorien. In S. Jaeger et al. (Eds.), *Beiträge zur Geschichte der Psychologie* (pp. 221–227). Frankfurt am Main: Peter Lang.

Strien, P.J. van (1996). Das Fortbestehen des "Leipziger Modells" in der modernen Psychonomie. In H. Gundlach (Ed.), *Untersuchungen zur Geschichte der Psychologie und der Psychotechnik* (pp. 105–115). München: Profil.

Strien, P.J. van (1997). The American "colonization" of Northwest European social psychology after World War II. *Journal of the History of the Behavioral Sciences, 33*, 349–363.

Strien, P.J. van (1998). Early applied psychology between essentialism and pragmatism: The dynamics of theory, tools and clients. *History of Psychology, 1*, 179–204.

Titchener, E.B. (1906). *Experimental Psychology; A manual of laboratory practice. Vol. I Qualitative experiments, Part 1. Student's manual.* London: Macmillan.

Woodworth, R.S. (1950). *Experimental psychology.* London: Methuen.

Yin, R.K. (1988). *Case Study research; design and methods.* London: Sage.

EXPANDING THE TERRAIN OF
CONSTRUCTING THE SUBJECT

THE RESEARCH RELATIONSHIP IN
INTERPERSONAL AREAS OF PSYCHOLOGY

Richard Walsh-Bowers[1]

INTRODUCTION

If one adopts Kurt Danziger's (1990, 1997) position that science is a discursive social practice and scientific work is an intrinsically public activity, then scientists' investigative practices and written discourse, that is, their rhetoric of scientific reports as well as "method talk," can be studied systematically. However, psychologists have taken for granted the historically constituted roles and functions of the parties involved in the investigative situation with humans (Danziger, 1990). Furthermore, with some exceptions psychologists have left unexamined the origins and social functions of two dimensions that bear directly on investigative practices: ethical guidelines for the conduct of psychological inquiry and report-writing norms for the production of scientific papers (Walsh-Bowers, 1995). In addressing the disciplinary significance of Kurt Danziger's (1990) *Constructing the subject*, I will connect his critical history of the social origins of psychological investigations with psychologists' conventions for ethical conduct with "human subjects" and for scientific report-writing.

During the 1960s and-70s some psychologists, operating independently from various vantage points, critically examined the quality of "experimenter-subject" relations in contemporary psychological research (Carlson, 1971; Giorgi, 1970;

Kelman, 1972; Kvale, 1973; Riegel, 1975; Schultz, 1969). Not all of these authors concluded that the conventional research relationship was fundamentally exploitive, but they agreed the data obtained under the typical investigative circumstances of controlling experimenters and inert human data sources were of suspect quality because of "subjects'" reactance to the depersonalizing investigative situation. Then critical histories of psychologists' investigative practices emerged, spanning a century of journal research reports and including the range of behavioral, interpersonal, and applied subdisciplines (Danziger, 1990; Morawski, 1988). This new historical approach showed that, decades ago, proponents of scientific rigor successfully imposed standards of decontextualized detachment for the investigative situation, minimizing the interpersonal context of conducting research to establish universal laws of behavior that transcended time, place, and person.

Paralleling the institutionalization of these investigative norms in the discipline were two significant developments (Walsh-Bowers, 1995). The first was the nearly total neglect of ethical considerations in human research until the 1960s. Until that point few psychologists seemed to care about the fact that, above all, the professional relationship between researchers and "subjects" was a social process of persons who shared a common humanity (Danziger, 1990). The second development in psychologists' culture that has dulled the sensitivity of generations to this relationship was the emergence of the American Psychological Association's (2001) *Publication Manual* in the 1950s, which led to the cultivation of "APA style" (Walsh-Bowers, 1999). Now in its 5th edition, the *Manual* has served to enculturate students and faculty in a particular set of standards for the composition of research papers that exclude attention to the relationship between investigators and research participants. These report-writing prescriptions are based on the assumption that genuine psychologists only conduct hierarchically organized and bureaucratically controlled experiments in which non-psychologist citizens only serve as "human subjects."

In this chapter I will expand Danziger's conceptual framework for understanding the socially constructed relationship between investigators and their assistants, on the one hand, with humans serving as sources of data, on the other hand. Influenced by feminist perspectives on methodology (e.g., Haraway, 1988; Harding, 1987) and by the concept of relationality, meaning the centrality of relationships in human life (Community Education Team, 1999), I use the term "research relationship" to emphasize the transactional nature of investigator-participant relations. This relationship encompasses three interrelated sets of disciplinary norms for conducting and writing about investigations: investigative roles and functions, standards of research ethics, and prescriptions for composing research papers for scientific journals (Walsh-Bowers, 1995). Historical inquiry in any one of these domains has important implications for the other two.

In expanding Danziger's framework my particular focus is on contemporary constructions of the research relationship in the interpersonal and applied subdisciplines of psychology, I will highlight the findings from archival studies of investigative practices in the interpersonal areas of psychology since 1939. Then I will introduce previously unpublished findings from a parallel study of research relationships in English-language European psychology journals and from my in-depth interviews with contemporary psychologists concerning the research relationship. On the basis of these sources of evidence I develop Danziger's explanatory model of the social systems in which the investigative situation has been embedded. I conclude by discussing the potential for changing the research relationship in psychology.

THREE FACETS OF THE RESEARCH RELATIONSHIP

ROLES AND FUNCTIONS

Social Origins

Through his extensive analysis of research published in the first 50 years of psychological journals Danziger (1990) demonstrated that the research relationship has taken several forms historically, associated with particular institutional contexts. (See also van Strien's chapter in this volume.) He based his archival research on the assumption that researchers' journal articles indicate how they actually constructed the relationship between the researcher and participant. Danziger illuminated the various possibilities for the conduct of psychological research in terms of five investigative roles and functions: designing an investigation, administering it, providing data, analyzing data, and writing about the investigation. He observed three distinct historical styles of conducting human research: (1) Wundt's model of shared research roles in laboratory experiments on consciousness, (2) the French medical hierarchical model of physicians' studies of patients, (3) the American large-group testing model patterned after Galton's hierarchical investigative practice. Theoretically, the five research functions can be shared by the parties involved in any particular psychological study. For example, in the Leipzig model the various research roles of designing and administering an investigation, serving as a data source, analyzing the data, and composing a research report were interchangeable among the members of the investigative team. Similarly, the research reports produced by Kurt Lewin and colleagues during his Berlin years were characterized by a descriptive style in which the relationship between investigator and participant was a central feature of the experiment reported. In North American psychology, however, these approaches to scientific composition and investigative conduct were relatively unusual.

Danziger found that by the 1920s there was little evidence in U.S. psychology journals of any one practicing the Wundtian model, which only survived in North American psychology in psychophysics. Ever since the early foundations for total investigator control of the research situation were established in the neo-Galtonian model, psychologists generally have assumed that human research should proceed only on the basis of a bureaucratic relationship with "subjects" (Morawski, 1988). Once investigative, ethical, and rhetorical norms for the research relationship were established, they were–and are–difficult to modify. These conventions have become deeply embedded in the ideology, mythology, and workaday practice of psychologists' scientific culture (Walsh-Bowers, 1995, 1999), even though there is no "scientific" reason, other than socially constructed tradition embedded in a staunch epistemological faith in objectivism, why psychologists should not have emulated the Wundtian and Lewinian models.

Mid-Century Practices

After World War II, with the rapid expansion of abnormal, developmental, and social psychology and the numerous applied subdisciplines in North America, especially clinical psychology, there were ample opportunities for psychologists to conduct research outside of the laboratory and in community settings, such as hospitals and schools, and to pay explicit attention to the quality of research relationships. In these investigative situations one might expect some flexibility of research roles. However, scrutiny of 3001 research papers published from 1939 to 1989 in 10-year intervals in one Canadian and seven U.S. psychology research journals dealing with the interpersonal areas of psychology indicated that the earlier norms of detached rhetoric and an objectified research relationship continued to prevail (Walsh-Bowers, 1995).

The analysis showed that, with few exceptions, investigators employed participants as data sources only. That is, almost invariably the traditional, hierarchical construction of roles prevailed. From start to finish, the researcher held hierarchical power over participants, whose only job was to provide data as the researcher saw fit. Researchers and assistants did everything else: planned, administered, analyzed, and reported psychological research. Furthermore, authors typically did not report consent, debriefing, or feedback; authors generally described participants but not data collectors and investigative settings, heavily used the term "subjects," and seldom acknowledged participants' contributions. Overall, depersonalized and decontextualized reporting was the norm. Moreover, there is no reason to believe that the underlying situation has changed since 1989 other than an increase in substituting "participants" for "subjects."

North American psychologists' habitual adherence to a research relationship of expert investigator and ignorant "subject" had a marked impact after World

War II on the rapidly expanding field of clinical psychology and ultimately on community psychology. When they adopted the "scientist-practitioner model" in 1949, clinical psychologists hoped to establish the scientific legitimacy of their profession for which identification with the hierarchical laboratory model of experimentation seemed essential. Subsequently, at their 1965 founding conference U.S. community psychologists, who were almost exclusively clinicians, also explicitly committed themselves to natural science psychology (Walsh, 1987). Simultaneously, the founders of this new subdiscipline initially promoted such ideals of scientific and professional practice as citizen participation, later expressed as empowerment. But there is very little evidence that community psychologists constructed research relationships that were essentially different from the detached experimental laboratory situation. Despite the founding value of community participation, community psychology authors gave little evidence, based on the first two decades (1973–1993) of research papers published in the two principal US community psychology journals, of cultivating collaborative research relationships with their participants and composing humanized and contextualized research reports (Walsh-Bowers, 2001). Furthermore, eminent community psychologists identified pervasive and powerful, historical constraints on community psychologists' actualization of their investigative ideals, including mainstream psychologists' beliefs about methodology (Walsh, 1987). To gain scientific legitimacy for their subdiscipline and to secure tenure personally community psychologists felt compelled to conform to conventional psychologists' conceptions of investigative conduct. These disciplinary standards of methodological rigor precluded attention to the research relationship. Moreover, the tradition of ignoring the research relationship that was established from the inception of the community psychology's journals set the tone for their second decade except for mere cosmetic changes, like using the term "participants" much more frequently than "subjects" (Walsh-Bowers, 2001).

RESEARCH ETHICS

Some social historians of psychologists' investigative practices take the position that methodological conventions are intertwined with ethical standards and norms for scientific report-writing in the discipline (Danziger, 1990; Morawski, 1988; Walsh-Bowers, 1995). But these other facets of the research relationship did not become explicit in North American psychologists' discourse until the 1950s. In fact, like other social and health scientists (Hobbs, 1965), U.S. psychologists did not produce full-fledged ethical guidelines for investigative conduct until 1963, over 70 years after they began their science. The fact that human psychological research takes place in a relationship between researchers and participants was subordinate to scientific psychologists' desire to project to the public an image

of professional responsibility, particularly in the wake of public exposures of scientific scandals attributable to U.S. and German scientists in the 20th century (Pettit, 1992). (See Adair, 2001, for an account of research ethics in Canadian psychology.)

The institutionalization of local ethical review boards by the 1980s reinforced the belief that research psychologists actually adhere to these ethical guidelines in their workaday investigative practice. Nevertheless, very few researcher-authors consider ethical standards important enough to describe or even simply report them in their research articles (Adair, 2001; Danziger, 1990; Morawski, 1988; Walsh-Bowers, 1995). But there are noteworthy reporting differences among the interpersonal subdisciplines. For example, although historically only a minority of authors among clinical and social researchers reported on the conditions of informed consent, social psychologists were even less likely to do so than clinical psychologists; moreover, social psychologists have been far more likely to use the now dubious but formerly *de rigeur* term "subjects" to designate the persons serving as data sources than clinical psychologists (Walsh-Bowers, 1995). Nevertheless, as a whole, psychological research has been characterized by the relative invisibility of ethical principles and guidelines for investigative conduct on the public face of investigative practices, namely, the empirical paper published in scientific journals.

My view of the conventional approach to research ethics in psychology as an active researcher and member of my own academic department's ethical review committee is that investigators treat ethical standards for the conduct of human research as if they were bureaucratic impositions on investigators' precious time and energy expended in managing their labor force, "subjects." The concept of a "research relationship" in which investigators have profound ethical responsibilities to their participants because they are engaged in a professional relationship apparently is meaningless to mainstream investigators. Psychologists seem to adopt a purely pragmatic position of coping with nettlesome ethical regulations to expedite the management of research participation and to ensure that the public views researchers as treating research participants with dignity. In the highly competitive U.S. and Canadian academic environments, few psychologists have adopted the position, which two prominent methodologists, Rosenthal (1994) and Rosnow (1997), have advocated, that sound ethical practices enhance any investigation's scientific merit.

When psychologists conduct research outside of the academy in applied settings, like a business or a school, they face complex social situations that demand more sophisticated public relations skills than those required for organizing the participation of university students in research on campus. Because of community resistance to psychologists' traditional investigative mode of "grabbing the data and running," that is, leaving the host setting with nothing meaningful for its participation, researchers have adapted by explaining to the authority figures in

the setting (e.g., the school principal and staff, and maybe the parents) what the investigators want to do so as to smooth the path for their research. The researchers might promise they will provide a "feedback" report tailored to the setting when they have analyzed and interpreted their findings. The researchers might fulfill their promise and send a report or return later to the setting to explain what they found out and how the findings might benefit the setting. But in this liberal approach, incorporating a glossy veneer of genuine participation for citizens providing data, the researchers do not share power with the employees or teachers and parents, only with the administrators or managers of the setting. The control over the investigative situation remains firmly in the hands of the investigative team just as it does in the intramural context.

Some psychologists place the fact of the research relationship in the center of ethical principles for research and draw from larger ethical values of democratic participation, justice, and compassion in the conduct of their inquiry (e.g., Walsh-Bowers, 1992; Wine, 1989). They stress that research should be a free exchange of resources in a relationship of shared power in which each party has rights and responsibilities, including as much participation in the research as the participants wish as well as development of educational benefits for the participants from the research findings (Rogers, 1997). When working with a group or organization, the democratic process entails the creation of a "research advisory committee" who represent the participants and are actively involved from entry to exit in the planning, doing, analyzing, interpreting, and writing of the research. This unconventional approach means *sharing* professional power and control over the entire research process, which contradicts bureaucratic traditions in the sciences, government, and business of professionals' paternalistic power over subordinates.

The Social Function of Research Ethics

In my view, the main reason mainstream psychologists support standards of research ethics is that, if investigators adhere to ethical practices that are normative in the discipline, they will facilitate participants' cooperation, which in turn will enhance high quality experimental research. That is, the willingness of people to serve as participants is connected to psychologists' projecting an ethically responsible image to the public. Indeed, many authors of writings about research ethics are explicit about how various threats to participants' privacy, for example, affect participants' compliance with researchers' goals (e.g., Stanley, Sieber, & Melton, 1996). That is, the authors argue that resolving these threats will improve the efficiency of the research enterprise. For instance, they advocate that more research on assurances of participants' privacy in research should be conducted to enhance public cooperation. But typically, mainstream psychologists do not question the suppositions of conventional investigative practice and of the traditional research relationship (e.g., Adair, 2001; Rosenthal, 1994; Rosnow, 1997).

This fact produces major conceptual problems within mainstream psychologists' discourse about methodology in human research. With some exceptions, the underlying contextual issues, such as the political economy of psychological research in terms of the pressures on academic psychologists to publish frequently in consensually-acknowledged superior journals, the power imbalance inherent in the conventional research relationship, and the social history of "constructing the subject," as Danziger (1990) put it, all are absent. In other words, the fact that the conduct of research has a social history is completely ignored. Similarly, psychologists could place the social significance of research ethics in the context of the decades-old literature on the epistemological foundations of psychological methodology, specifically, the problematic nature of objectivity in psychological science and the subject-object relationship (Manicas & Secord, 1983; Haraway, 1988; Harding, 1987). But typically psychologists addressing methodological principles and practices do not question their epistemic base; on the contrary, they celebrate the discipline's modernist foundations in the tenets of realism, determinism, and reductionism (e.g., Madigan, Johnson, & Linton, 1995).

My interpretation of the conventional construction of research ethics is that it exists to legitimize conventional methodological practices. It is true that since the emergence of formal ethical guidelines psychologists have advanced the lofty ethical values of respecting the dignity of each person and promoting human welfare. But I would argue that from their inception ethical standards for research have projected to the public a patina of concern for the welfare of "subjects," while simultaneously strengthening psychologists' administrative control over the investigative situation. Conventional constructions of research ethics enable psychologists to rationalize the passivity and malleability of research participants in the conduct of inquiry; that is, psychologists preserve intact the dominant-subordinate relations intrinsic to their preferred investigative model of "experimenters" and "subjects." In short, codification of research ethics has facilitated scientific psychologists' capacity for effectively managing the impression of ethical practices while conducting investigative business as usual in the production of marketable psychological research. Thus, the specific categories of research ethics (e.g., voluntary informed consent) serve as the public relations grease for the assembly line of the experiment, conducted within an authoritarian relationship between the investigative team and participants. It is in this relationship mode that psychologists easily can rationalize the use of deception (e.g., Adair, 2001; Stanley et al., 1996).

In conclusion, psychologists cannot have meaningful conversations about research ethics without explicitly discussing the implications for research methods, whether qualitative or statistical. There are alternatives, however, to the conventional mode of conducting research. Like some other psychologists, I would argue for a moral imperative for reconstructing the research relationship. A democratized research relationship, in which ethics and methodology would be interconnected with humanized, contextualized report-writing, is a constructive alternative to mainstream psychology's tradition. In a democratized, bilateral relationship

the parties can engage in a truly processual, participatory ethics in which consent becomes an open process of on-going communication (see also Holzkamp, 1985/1991). In community psychology and feminist psychology such an integration has already emerged to some extent. It remains to be seen whether conventional psychologists become cognizant of this trend and then respond to the challenge of reconstructing the research relationship in word and deed.

Scientific Report-Writing in Psychology

Rhetoric in Science

According to scholars of scientific writing, making scientific knowledge begins and ends with persuasion (e.g., Gross, 1990; Taylor, 1996). That is, scientists persuade themselves of a particular position with private language, they empirically investigate this notion with private and public language, and then they attempt publically to convince their scientific peers that their claim to knowledge is valuable. Thus, the core of science involves multiple interpersonal uses of language to persuade others. The art of persuasion, of course, is rhetoric. The fact that a scientific paper seems to be free from emotional appeal does not mean that the paper is "neutral" in reality (Bazerman, 1988). Apparent neutrality is the author's pose of scientific detachment to enhance credibility within her or his scientific community as a detached observer.

By the 20th century conventional report-writing in science consisted of adherence to the rhetorical values of precision, logic, order, and clarity. However, stereotypical scientific writing can be poor writing from the point of view of good English. For example, the common phrase, "It was found that," illustrates four points of bad writing: needless words, pomposity, vagueness, and passive voice. Moreover, authors of contemporary manuals of scientific writing tend to ignore the realities of rhetoric and to pretend that the scientific writer should be completely objective and merely hold a mirror up to nature (e.g., Thaiss & Sanford, 2000). The common belief is that scientists write objectively, that is, their use of language in their formal journal articles only reflects dispassionate detachment. But the reality is that scientists try to *appear* rational, when in fact they rely heavily and often unconsciously on rhetorical devices in their writing to persuade their readers that their work is better than their competitors. Moreover, many scientists will privately admit that they experience strong social pressure to be on top in their field and to create the illusion in their writing that they are (Gross, 1990).

APA Style

The public face of psychological inquiry is most apparent in empirical journal papers, which authors construct according to taken-for-granted disciplinary traditions (Danziger, 1990). Psychologists have practiced a particular type of scientific

rhetoric at least since the 1920s that complemented the discipline's behaviorist orientation (Bazerman, 1988). By this point in the North American discipline's history the canonical research paper became one in which the author was invisible, the research participants were objectified, and the data and generalizable conclusions were salient within a standard format of Introduction, Method, Results, and Discussion. Rooted in the laboratory reports of the 19th century physical sciences, the format for the experimental paper requires that authors provide brief coverage of previous research, then the psychological theory and hypotheses; followed by the details of the investigative procedure and the statistical results; then interpretations of the research findings and speculations about theory and possible future research. In contrast, the experimental papers produced by Wilhelm Wundt and his associates employed a more fluid scientific rhetoric, which reflected the much more flexible relationship between data administrators and data sources practiced in the Leipzig model (Danziger, 1990). But by the 1930s psychologists standardized an objectified type of scientific rhetoric (Bazerman, 1988) and consciously prescribed explicit disciplinary codes for the composition of research papers, which were subsequently formalized in the first edition of the American Psychological Association's *Publication Manual* in 1952. These codes are known as "APA style," although they pertain to both the form of research papers as well as the writing style (Walsh-Bowers, 1999). Since 1952 and its later editions of 1974, 1983, 1994, and 2001 the *Manual* has contained specific guidelines for the composition of research reports for just one kind of research method, namely, the quantitative laboratory experiment.

Psychologists have been deeply committed to the APA tradition of depersonalized, decontextualized journal articles, because, as its defenders asserted (Madigan et al., 1995), "APA style" matches disciplinary intentions to establish universal laws of behavior that transcend specific persons and social historical contexts. The rhetoric of behaviorism has shaped conventions of scientific report-writing in psychology to such an extent that psychologists and their undergraduate and graduate students assume that this form of composition and style is the only way to compose research papers for journals.

Scrutiny of the *Manual*'s prescriptions reveals that writers should refrain from acknowledging the interpersonal nature of human research (i.e., the research relationship) and that the writing style should be as objectively detached as possible (Walsh-Bowers, 1999). The authors of the *Manual* direct readers to employ an impersonal, detached, objective, and rational writing style. Personal information and feelings should be absent, as research psychologists believe the subjective is appropriate for literature, not for science. Authors of manuals on psychological report-writing echo the *Manual* in directing students to compose the research paper as if they were solving a cognitive problem, connecting problem-solution to evaluation of the outcome (e.g., Thaiss & Sanford, 2000). But completely ignored in this standardized formulation is the scientist-author solving the investigative problem.

As to the other party to the research relationship, the participants, the *Manual* authors direct writers of experimental papers to report sufficient, relevant demographic characteristics of the participants to ensure that the claim to universality of the reported findings is robust. Consequently, psychology research articles usually contain only sketchy information about participants and their context; for instance, authors of 28% of the empirical papers published as recently as 1989 in the *Journal of Personality and Social Psychology* did not even indicate the gender of the participants (Walsh-Bowers, 1995). In addition, authors typically do not provide information about the conditions of informed consent for participation or any feedback on the findings. Furthermore, information about the researchers' characteristics generally is absent, as if the investigation had been conducted by an automaton. Psychologists' empiricist assumption is that the data simply should speak for themselves.

Alternative approaches to this uniformity of depersonalized and decontextualized report-writing are relatively rare in psychology, although there always have been a few authors in the interpersonal areas who composed their research reports in such a way as to incorporate information about the nature of the relationship between investigator and participant (Walsh-Bowers, 1995). Moreover, some intentional change in subdisciplinary preferences has emerged in feminist psychology and community psychology. For example, for over a decade three community psychology journals (the *American Journal of Community Psychology*, *Canadian Journal of Community Mental Health*, and the *Journal of Community Psychology*) have stipulated in their instructions to contributors that authors should describe the research relationship directly in their articles.

Because it contains only one research report for emulation—the quantitative laboratory experiment—the *Manual's* prescribed structure also shapes the content of the psychological knowledge reported (Bazerman, 1988). But the required format and style arguably are ill-suited to reporting human psychological research derived from real-life contexts, particularly when the investigations are based on methods other than the experiment. For example, despite the fact that they are increasingly popular in such areas as counseling, community, developmental, and personality psychology, qualitative research methods, which lend themselves to innovative compositional forms and styles (Richardson, 1994), remain invisible in the most recent edition of the *Manual*. "Case studies," for instance, merit only cursory coverage in the opening chapter.

Scientistic Rhetoric

In my opinion, there is a kind of scientistic fundamentalism at work in the simplistic obedience to "APA style" that psychologists cultivate throughout the social systems of the discipline, but most evident in the preparation and review of papers intended for journal publication. In effect, the *Manual* has functioned

as a quasi-Bible for authors as well as for journal editors and reviewers (Walsh-Bowers, 1999). The *Manual's* prescriptions became unquestioned Holy Writ and "APA style" is assumed to be the only legitimate way of composing research papers. No alternative rhetorical norms are presumed to exist. The core doctrine in psychologists' "Bible" is that no description of the interpersonal nature of human research (i.e., the research relationship) is necessary in an acceptable research paper and that the writing style should be as objectively detached as possible. Moreover, the codification of "APA style" is overtly linked to the maintenance of the orthodox research relationship of psychologists' hierarchical power and to the enculturation of psychology students in these compositional and investigative traditions (Madigan et al., 1995).

The meta-message of the discipline is clear: "The *Manual* reflects our firm beliefs about what constitutes proper methodology. If you do not adhere to our Bible, you will lose your soul to unobjective writing that begets unscientific knowledge, and we will not publish your work." But rather than unreflective conformity with fundamentalist codes of purportedly correct writing, psychologists need rhetorical flexibility so that how they compose their empirical papers, whether reporting experiments or qualitative methods, corresponds to the complexity and inherently relational nature of socially contextualized psychological phenomena.

Yet in every edition the *Manual* has minimized attention to the nature of the research relationship. This fact, I would argue, has profound implications for standards of methodology, inasmuch as the quality of the research relationship is crucial to the quality of the data obtained in any given investigation of human psychological phenomena. This relationship is particularly important in the interpersonal areas of psychological research, such as abnormal, developmental, social, and personality, not to mention the applied subdisciplines like clinical, community, and educational psychology, in which issues of method and research ethics are intertwined with authors' reportage. Thus, even some mainstream psychologists have argued that authors should welcome the opportunity to implement ethical standards of research and to report, for example, the conditions of informed consent, so as to facilitate replication by other investigators (e.g., Adair, 2001; Blanck, Bellack, Rosnow, Rotheram-Borus, & Schooler, 1992; Rosnow, 1997).

Colonization of English-Language European Psychology

To what extent have authors of empirical reports published in English-language European journals of psychology adopted the conventions of the *Manual* for actual methodological conduct and for writing style? Has "APA style" predominated in these journals? As is evident below, the North American model of research relationships definitely has influenced investigative practices in English-language European psychology at least in the interpersonal areas of the discipline.

I reviewed all (N = 446) research reports published over 10-year intervals from 1966 to 1996 in five English-language European journals of psychology, primarily dealing with the interpersonal areas of psychology (e.g., clinical, developmental, and social psychology). The clear majority of first authors identified their affiliation as European, although a minority were affiliated with US or Canadian institutions.

The findings were as follows:

1. There was no evidence of researchers sharing research roles with participants, who played only one role, data source, with one minor exception.
2. Most authors did not report even the scantiest information about participants' voluntary, informed consent, although there were differences among the journals.
3. No authors indicated providing feedback to their participants on the findings of their investigations, but some authors in social psychology journals did report that they debriefed their participants.
4. Authors were much more likely to specify the gender of their participants than of the persons administering the data collection, and authors typically rendered the data collectors invisible.
5. Authors relied on the term "subjects" with or without other terms (e.g., "students," "participants," "parents"), although usage varied substantially by year and journal sampled.

In sum, the vast majority of empirical papers published in the interpersonal areas of psychology in English-language European psychology journals from 1966 to 1996 showed the predominance of "APA style." That is, they contained objectified rhetoric like that prescribed by successive editions of the *Manual*, which minimizes attention to the research relationship between investigators and participants. Thus, although no simple, cause-and-effect relationship is identifiable, the findings suggest that a kind of U.S. hegemony over investigative conduct and report-writing has been at work, at least indirectly, in research reports authored by European psychologists who publish in English. Even though the English-language European journals sampled had explicit, unique compositional criteria for their authors, implicitly the journals adopted the prescriptions of the *Manual* for objectivistic format, content, and rhetoric, rationalized as essential for the discipline's epistemology (Madigan et al., 1995), with no major exceptions. This general trend mirrors the conventional configurations of hierarchical research relationships and depersonalized, decontextualized scientific rhetoric across nearly a century of North American psychology (Danziger, 1990; Morawski, 1988; Walsh-Bowers, 1995, 1999).

Ironically, "APA style" might have been appropriate, because there was not much particularly European, or socially contextualized, about these reports. Rather,

the authors seemed to have pursued the path of producing generalizable findings to demonstrate universal laws of human behavior that transcend social location and the particular human relationships within the investigative situation that produced the findings. This conclusion should not be surprising, given that scientific psychology became universalized U.S. psychology by the 1930s (Danziger, 1997), as exemplified by Dutch psychology post-World War II (van Strien, 1997). Thus, a kind of U.S. colonization of English-language European psychology occurred.

THE CURRENT SITUATION

INTERVIEWS OF RESEARCHERS

What is the contemporary status of these disciplinary codes of investigative conduct that permeate the production of publishable papers for research journals, undergraduate and graduate curricula, and thesis supervision? A valuable perspective on the present and future status of the research relationship in psychology is investigators' experience as researchers, authors, teachers, research supervisors, and students. Researchers' accounts can illuminate the link between the immediate social situation of investigator and participant, on the one hand, with the complex, layered dimensions of institutionalized scientific psychology that envelop the research relationship, on the other hand. In addition to methodological, ethical, and reporting norms, the structures, mores and ideologies of scientific psychology as a social institution also mould workaday investigative practices (Danziger, 1990). These ideological and structural systems, which typically function as covert features of the research landscape, include the historical place of the research relationship in scientific psychology; epistemological assumptions about making scientific knowledge; the enculturation of students in investigative customs, mediated by course instructors and research supervisors; the function of research productivity within the academic reward system and the effects of the socioeconomic reward system for faculty on sensitivity to research relationships; methodological criteria promoted by funding sources, journal editors, and grant and journal reviewers; psychologists' beliefs, feelings and wishes about what constitutes rigorous methodology; and the potential for changes in the discipline that could legitimize fostering research relationships.

To better understand these systemic influences on socially constructing psychological knowledge I interviewed 36 active researchers (11 graduate and 3 postdoctoral students and 22 faculty) from diverse subdisciplines in Canadian psychology on the past, present, and future of the research relationship. The 14 (9 women and 5 men) students came from 5 Canadian universities. Their research interests spanned the clinical, cognitive, community, cultural, developmental, health, historical-theoretical, personality, and social areas of psychology. The

22 faculty members (15 men and 7 women) represent 7 Canadian universities. Their research areas encompassed clinical, cognitive, community, counseling, developmental, educational, historical-theoretical, industrial-organizational, personality, and social. The faculty are active contributors to the literature in their respective subdisciplines, they supervise undergraduate and graduate students' research, and most had served as journal and grant reviewers; furthermore, 3 of them had been department chairpersons and 6 had served as journal editors. A slight majority of the faculty and student participants practiced alternative research methods.

Using a semi-structured format in conversational interviews that varied from 45 to 120 minutes, I asked the interviewees how they learned to relate with participants, to apply ethical standards, and to write scientific papers. Then I asked them to comment on the applicability of investigative norms for their particular research areas and on the future of the research relationship.

Findings

The interviews showed that the intertwined social systems within which psychology students and faculty practice their research might be taken for granted and be rarely investigated, but they are nonetheless just as constitutive of the investigative situation as the immediate relationship between investigators and research participants. Many participants reported their experiences with conventional methodological standards applied by editors and reviewers of research journals in psychology, resulting in opposition to publishing papers in which authors describe their research relationships. Participants also referred to the premium placed by peers on tenure and promotion committees on experimental papers published in peer-reviewed journals considered to be top-rank.

Diverse participants made it clear that the academic reward system as traditionally constructed in psychology militates against faculty and student attending to the quality of research relationships. The socioeconomic contingencies for faculty are linked to conformity with the *Manual's* prescriptions for quantitative experimental report-writing in that, as one postdoctoral student observed, receiving tenure, promotion, and grants partly depends upon publishing conventional research with the conventional format and style in conventional journals. Several interviewees stated that psychological research typically is heavily reliant on implementation of sophisticated statistical techniques, motivated by researchers who feel pressured to produce marketable empirical papers so as to ensure tenurability and promotion.

Publication pressures adversely affect the quality of research relationships, particularly in the interpersonal and applied areas, as the findings from a study of eminent community psychologists indicated (Walsh, 1987). Mainstream investigators feel as compelled to "grab the data and run" when conducting studies in non-academic locations as they do in managing masses of undergraduate "subjects"

through the production-line of university-based investigations. Graduate students rapidly learn both the mores and the social systemic contingencies enveloping investigative practices. A clinical student, for example, doing a cognitive dissertation, observed that experimentation within pre-existing narrow theoretical domains facilitates the production of publishable papers, which research funding and the training subsystem for supporting graduate students reinforces.

In addition to the pressures of the academic reward system, psychologists' investigative practices are formed by an ideological vision of objectively detached investigative conduct in which research relationships are irrelevant. Those students and faculty who pursued mainstream psychological research took the modernist epistemological foundations of the field at face value. They seemed to unreflectively reproduce disciplinary standards for objectivistic knowledge-making, for which bureaucratized relationships with "human subjects" are essential. In reporting how they carefully prepared their "subjects" (the term they most often used) for limited participation, the unquestioned *modus operandi* of mainstream faculty and student researchers was hierarchical data-extraction usually in quantitative experiments. The interviews clearly indicated that traditional researchers have taken for granted conventional investigative roles and functions, ethical guidelines, and report-writing. In fact, the very notion of a "research relationship" seemed to baffle many of them. The interview participants' anticipations of the future for investigator-participant relations were similarly myopic. For example, postmodernist and feminist principles of intersubjectivity and investigator reflexivity, with possibilities for shared research roles, only surfaced among those faculty and students who identified with alternative research positions and qualitative methods, like discourse analysis, grounded theory, or narrative analysis.

Regarding sensitizing students to the research relationship, faculty have focused undergraduate and graduate education on socialization in the quantitative laboratory experiment, and faculty research supervisors who foster research relationships are rare. This situation is arguably the most formidable obstacle to raising students' awareness of the range of possibilities for investigative roles and functions, research ethics, and report-writing. Students require role models of scholars who demonstrate sensitivity to research relationships in investigative practice and in print whom students can emulate.

The interview findings suggest that the potential for fundamental change in psychologists' consciousness about research relationships is dubious. As one senior researcher who is committed to fostering research relationships and to alternative research methods observed, "It's like turning a supertanker around." Similarly inclined faculty and most of the students indicated that for disciplinary legitimation of research relationships to occur systemic changes are necessary in journal policies and practices, the *Manual*, and psychology curricula and research mentoring. However, as many participants noted, there are major sources of ideological resistance to these institutional changes among many mainstream psychologists,

as sensitivity to research relationships threatens their foundational conceptions of psychological science. Consequently, the efforts of some psychologists to expand traditional boundaries in conducting and writing about their investigations and to broaden the education of undergraduate and graduate students in psychological research practices are inhibited by the powerful undertow of resistance to change in the discipline. On the other hand, there was some evidence for generational change in that the postgraduate fellows and graduate students as a group seemed relatively open to learning about research relationship issues, even those whose undergraduate and graduate education in psychology had not covered this concept nor alternative methodologies.

Conclusions

If these findings have any transferability to other North American psychologists, it appears that typical mainstream psychologists, as well as graduate students absorbing the norms of investigative conduct, only conceive of research as data-extraction from relatively inert "subjects." Consequently, extant ethical and report-writing prescriptions suit the production of marketable psychological research quite well. These disciplinary codes serve to protect "business as usual," that is, investigator power and control of the research relationship in all its aspects and functions. Moreover, in journal papers for public consumption and in workaday relations with "human subjects," investigator domination is likely to prevail in the future, assuming these researchers' practices mirror the discipline's scientific vision. Overall, their interview accounts directly reflected Danziger's (1990) point that, "It is precisely the social aspects of scientific practice that are systematically excluded from practitioners' discussions about methodology" (p. 13). That is, conventional psychologists take for granted the conventional construction of research roles, ethical guidelines, and report-writing.

SUSTAINING CRITICAL HISTORY OF THE RESEARCH RELATIONSHIP

The Research Relationship in Social Context

The interviews and various archival studies described above permit the development of a critical theoretical framework for the research relationship. Danziger (1990) placed the immediate social situation of a psychological investigation at the core of multi-layered systems, specifically, the research community with its norms for acceptable knowledge. This latter system in turn is embedded in a professional environment of research institutions, funding sources, scientific-professional bodies, and societal consumers of research. In addition, he situated research reports on the boundary between the immediate research relationship of the two parties

and the scientific audience of editors and reviewers, readers, and textbook writers. Danziger also referred to a fourth, all-inclusive system—the socio-political—that impinges upon investigators' constructions of the subject.

In revising this conceptual framework to encompass a continuum of possible investigative social arrangements in contemporary psychology, including those in applied settings, I stress the dialectical nature of the observer-observed relationship and the social historical contexts of both parties to that relationship. To illustrate, the immediate context of researcher and participant occurs within communities of knowledge-seeking, that is, participants seek information about themselves as psychological beings or as constituents of community settings, and both parties in the research relationship function within institutional and community environments. These layers, in turn, are embedded within a macro, socio-political context that includes ideological formations and legitimized social practices pertaining to usually implicit notions of authority relations endemic in society (e.g., between managers and workers) and ethical values (e.g., an orientation to social justice vs. cost-benefit analyses of social relations). Here is where tacit assumptions about professional identity as a scientist and the expertise of researchers operate. Psychology students, for example, quickly learn that their future professional identity is bound up with investigator power and control over the investigative situation.

The second distinguishing feature of a dialectical framework for the research relationship is the notion of a continuum of relationships, depending upon the institutional setting. The typical, micro-investigative context has been the university in which unorganized individual students serve as "subjects." A similar relationship exists regarding captive populations, such as mental patients. A second type of research relationship occurs in community agencies and institutions, such as day care centers, and in businesses. Managers and professional authorities in these settings serve as intermediaries for researchers seeking access to "subjects;" for example, investigators must negotiate with school principals, teachers, and parents to study children's behavior. Another type of relationship can occur between researchers and an entire organization in business or the community, in which both parties acknowledge mutuality of interests and plan applications of the research to the organization itself.

The third feature of this revised model is the function of journal research reports. Research papers represent consensual notions of "good" science to the community of knowledge-seekers, mirroring socially acceptable roles for researchers and participants. But research reports serve other social purposes. Besides conforming with institutionalized rhetorical norms, authors strive to impress journal editors and reviewers and then a larger audience of potential consumers of their research. Moreover, psychologists use their reports as commodities in their workaday marketplace to advance their careers. At the broadest systemic level, psychologists' scientific papers reflect both typical authority relations in the culture, enacted by bureaucratized research relationships, and the special status of psychologists

in the public's eyes. Thus, standardized research reports enculturate researchers themselves, their peers, student and professional consumers, and the public.

THE POTENTIAL FOR CHANGE

Conceptions of the conduct of psychological inquiry seem to be shifting, thanks to the efforts of advocates of alternative investigative practices. Already editors of a few journals are consciously inviting and publishing qualitative research papers. Moreover, in North American journals there have always been some exceptional instances of humanized reporting and even relatively non-hierarchical investigative practice (Walsh-Bowers, 1995). In other words, researchers have had more freedom than they realize to employ alternative research relationships and to create socially contextualized psychological knowledge in their journal articles. On the other hand, there have always been systemic limits to such practices. In this section I specify recommendations for changing the research relationship in investigative practice and in print, but then I identify sources of resistance to these changes that threaten the institution of scientific psychology and the professional identity of scientific psychologists.

Recommendations

Some psychologists advocate greater formal attention to the research relationship in research papers to heighten researchers' sensitivity to important matters of research ethics and to improve the quality of future research (e.g., Walsh-Bowers, 1995, 1999). Accordingly, psychologists could take innovative steps and integrate method and ethics in their research reports by adopting the concept of the research relationship. When journal editors, their reviewers, and formal instructions to contributors to a journal join in by supporting authors' innovations, conducting research relationally and writing about it humanely gain disciplinary legitimacy. Journal editorial boards in their "instructions to contributors" could advise authors making submissions to provide fuller information about the research relationship in their manuscripts than is prescribed by the *Manual*, and editors and reviewers could ensure that authors do so. Correspondingly, the authors of the *Manual's* next edition could incorporate qualitative methods as well as the flexibility of report-writing and attention to research relationships that alternative methods demand.

Alternative guidelines for writing research articles, in fact, have emerged that serve to integrate considerations of method and ethics within a humanized and contextualized approach to scientific rhetoric. As reported previously, one Canadian and two U.S. community psychology journals instruct contributors to describe in their empirical papers the nature of the research relationship established with the participants, including conditions of consent and feedback on findings, and to specify the participants' and settings' characteristics. A consciously participatory

mode of investigative roles and functions, for instance, readily lends itself to relational reporting. In concert with these systemic changes a shift in undergraduate and graduate curricula would be necessary to ensure that students' enculturation in investigative mores includes sensitivity to research relationships in practice and in print. As the student-participants in the above-mentioned interviews made clear, they received no education in research relationships unless they were exposed to alternative research practices, such as qualitative methods. Relating and writing humanely should have pride of place in the education of future psychologists, and undergraduate and graduate students need positive role models to emulate so that subsequent generations of investigators can foster the research relationship.

Inhibitions

The historical record strongly suggests that the investigative, ethical, and compositional traditions established early in the journals are likely to persist, unless psychologists become conscious of their taken-for-granted practices and decide to change them. Furthermore, there are powerful cultural and institutional forces in our discipline that inhibit any recommended systems-level changes. In attempting to transform the research relationship psychologists have to address both its spirituality (i.e., its myths and internalized ideology) as well as its socially constructed, historically contingent external forms. Deeply internalized modernist norms are powerfully present among psychologists concerning what constitutes "rigorous research," which are difficult to transcend. Such inhibitory factors include psychologists' intense, unflagging desire to emulate the so-called "hard" sciences, which is sustained by the pervasive influence of the *Manual* (Walsh-Bowers, 1999). Psychologists' fundamentalist faith in the prescriptions of scientist "APA style" militates against substantive change to guidelines for writing empirical reports. It remains to be seen whether psychologists will have the courage to acknowledge the socially constructed nature of conducting research and composing empirical papers and then to permit if not support those peers who are so inclined to embrace unconventional forms of investigative and compositional practice.

The central issue is not whether the predominance of "APA style" as an ideology for investigative practices is an instrument of hegemonic U.S. psychology. It is tempting to think so, because U.S. psychology serves as the colonizing, reference culture for the globe's psychologists and the colonized submit to its language and methods (van Strien, 1997). For example, psychologists have relegated the shaping of research relationships to the APA *Publication Manual*, among other tools, to maintain a universalized psychology modeled on modernist natural science. Rather, the issue is the nature of the rationales and supporting disciplinary practices and codes that psychologists have constructed historically in relation to the investigative situation. Cultivating a critical discourse on the research relationship is both theoretically and practically valuable, because the quality of this relationship

affects the quality of the data derived from it, which cuts to the very heart of our knowledge-claims as psychological scientists. Psychologists could intentionally develop emancipatory alternatives that dethrone scientists' privileged location and create equality of voice in attempting to understand socially contextualized human experience (Holzkamp, 1985/1991; Sampson, 1991; Wine, 1989). Psychologists could transform the conventional research relationship of domination into a liberatory relationship in all three aspects: investigative roles and functions, the ethics of investigative conduct, and norms for composing empirical papers.

NOTE

[1] I am grateful to the editors and Pieter van Strien for their comments.

REFERENCES

Adair, J. G. (2001). Ethics of psychological research: New policies; continuing issues; new concerns. *Canadian Psychology, 42*, 25–37.

American Psychological Association. (2001). *Publication manual of the American Psychological Association* (5th ed.). Washington, DC: Author.

Bazerman, C. (1988). *Shaping written knowledge: The genre and activity of the experimental article in science*. Madison, WI: The University of Wisconsin Press.

Blanck, P. D., Bellack, A. S., Rosnow, R. L., Rotheram-Borus, M., & Schooler, N. (1992). Scientific rewards and conflicts of ethical choices in human subjects research. *American Psychologist, 47*, 959–965.

Carlson, R. (1971). Where is the person in personality research? *Psychological Bulletin, 75*, 203–219.

Community Education Team. (1999). Fostering relationality in implementing and evaluating a collective theatre approach to preventing violence against women. *Psychology of Women Quarterly, 23*, 97–111.

Danziger, K. (1990). *Constructing the subject: Historical origins of psychological research*. Cambridge, England: Cambridge University Press.

Danziger, K. (1997). *Naming the mind: How psychology found its language*. London: Sage Publications.

Giorgi, A. (1970). *Psychology as a human science*. New York: Harper & Row.

Gross, A. G. (1990). *The rhetoric of science*. Cambridge, MA: Harvard University Press.

Haraway, D. (1988). Situated knowledges: The science question in feminism and the privilege of partial perspective. *Feminist Studies, 14*, 575–599.

Harding, S. (1987). *Feminism and methodology: Social science issues*. Bloomington, IN: Indiana University Press.

Hobbs, N. (1965). Ethics in clinical psychology. In B. B. Wolman (Ed.), *Handbook of clinical psychology* (pp. 113–127). New York: McGraw-Hill.

Holzkamp, K. (1991). Experience of self and scientific objectivity. In C. W. Tolman & W. Maiers (Eds.), *Critical psychology: Contributions to an historical science of the subject* (pp. 65–80). Cambridge, England: Cambridge University Press. (Original work published in 1985)

Kelman, H. C. (1972). The rights of the subject in social research: An analysis in terms of relative power and legitimacy. *American Psychologist, 27*, 989–1016.

Kvale, S. (1973). The technological paradigm of psychological research. *Journal of Phenomenological Psychology, 3*, 143–159.

Madigan, R., Johnson, S., & Linton, P. (1995). The language of psychology: APA style as epistemology. *American Psychologist, 50*, 428–436.

Manicas, P. T., & Secord, P. F. (1983). Implications for psychology of the new philosophy of science. *American Psychologist, 38*, 399–413.

Morawski, J. G. (Ed.). (1988). *The rise of experimentation in American psychology*. New Haven, CT: Yale University Press.

Pettit, P. (1992). Instituting a research ethics: Chilling and cautionary tales. *Bioethics, 6*, 89–112.

Riegel, K. F. (1975). Subject-object alienation in psychological experiments and testing. *Human Development, 18*, 181–193.

Richardson, L. (1994). Writing: A method of inquiry. In N. K. Denzin & Y. S. Lincoln (Eds.), *Handbook of qualitative research* (pp. 516–529). Thousand Oaks, CA: Sage Publications.

Rogers, T. B. (1997). Extending the CPA Code of Ethics: A research participant's Bill of Rights. *History and Philosophy of Psychology Bulletin, 9*, 3–15.

Rosenthal, R. (1994). Science and ethics in conducting, analyzing, and reporting psychological research. *Psychological Science, 5*, 127–134.

Rosnow, R. L. (1997). Hedgehogs, foxes, and the evolving social contract in psychological science: Ethical challenges and methodological opportunities. *Psychological Methods, 2*, 345–356.

Sampson, E. E. (1991). The democratization of psychology. *Theory & Psychology, 1*, 275–298.

Schultz, D. P. (1969). The human subject in psychological research. *Psychological Bulletin, 72*, 214–228.

Stanley, B. H., Sieber, J. E., & Melton, G. B. (Eds.) (1996). *Research ethics: A psychological approach*. Lincoln, NB: University of Nebraska Press.

Taylor, C. A. (1996). *Defining science: A rhetoric of demarcation*. Madison, WI: University of Wisconsin Press.

Thaiss, C., & Sanford, J. F. (2000). *Writing for psychology*. Boston: Allyn and Bacon.

Van Strien, P. J. (1997). The American "colonization" of Northwest European social psychology after World War II. *Journal of the History of the Behavioral Sciences, 33*, 349–363.

Walsh, R. T. (1987). The evolution of the research relationship in community psychology. *American Journal of Community Psychology, 15*, 773–788.

Walsh-Bowers, R. (1992, August). *Democracy in American psychological research practice*. Paper presented at the meeting of the American Psychological Association, Washington, DC.

Walsh-Bowers, R. (1995). The reporting and ethics of the research relationship in areas of interpersonal psychology, 1939–89. *Theory & Psychology, 5*, 233–250.

Walsh-Bowers, R. (1997, July). *The research relationship in English-language European psychology journals*. Paper presented at the Fifth European Congress of Psychology, Dublin.

Walsh-Bowers, R. (1999). Fundamentalism in psychological science: The *Publication Manual* as Bible. *Psychology of Women Quarterly, 23*, 375–393.

Walsh-Bowers, R. (2001, June). *The evolution of the research relationship in community psychology revisited*. Paper presented at the biennial meeting of the International Society for Theoretical Psychology, Calgary.

Wine, J. D. (1989). Gynocentric values and feminist psychology. In A. R. Miles & G. Finn (Eds.), *Feminism: From pressure to politics* (2nd rev. ed.) (pp. 77–97). Montreal: Black Books.

ON CULTURAL HISTORY AS TRANSFORMATION—OR, WHAT'S THE MATTER WITH PSYCHOLOGY ANYWAY?[1]

Betty M. Bayer

On Kurt Danziger's retirement from York University, a panel was arranged for the Canadian Psychological Association to recognize his contributions to the discipline of psychology and to the panelists' scholarly and intellectual development as academic psychologists. Our task was to focus on a piece of his work that had played some central role in our own thinking. Having a few years into my tenure track job and having had a remarkable two years prior to that as a Social Sciences and Humanities Research Council of Canada postdoctoral fellow, I was thrilled with the opportunity, as a fairly new (and optimistic) assistant professor, to offer reflection on what I saw as joint transformative powers of newer critical histories and feminism on psychology. These twinned forces of transformation had been addressed by Danziger (1994) in a paper he had delivered a few years earlier, and published later as "Does the history of psychology have a future?" For our panel, I entitled my paper "Life part way in, part way out"[2] to reflect that in-between place of history and feminism within psychology, and how this position might afford (or so I thought) the potential to transform the discipline (see Bayer, 1995). Sometime after, I learned from Danziger that he had tackled the question of the relation between marginality and disciplinary transformation in a paper he gave at Cheiron Europe in the early 1990s, "In praise of marginality" (1992). Here he attended to three sites of marginality as carrying the potential

for transformation—"the marginality of Psychology in the Netherlands[,] . . . the marginality of Cheiron Europe . . . [and] that of the modern historian of psychology" (p. 7). The transformative potential of the latter site was conceived of as "a space at the margins of science and history that can be usefully filled by historians whose professional affiliations are not with history but with a particular scientific discipline" (p. 8). Much in line with my own thinking, these works directed themselves to the insider-outsider status of history and of feminism as wellsprings of transformation for psychology as for other disciplines. Few of us questioned this kind of formulation at the time, and many continue to adhere to a view of the margins as disruptive or transgressive forces that somehow bring about transformation. This chapter picks up on the trail of the quest for transformation in psychology, examining further the relation between changing knowledge-making practices and transformation of the discipline. Because so many of us assumed that remaking how we produce knowledge would eventuate in a changed discipline, my examination of this set of expectations begins by putting in context the matter of disciplinary change from roughly Post-World War II on. The topic perforce encompasses numerous concerns, from those of epistemology and ontology through to more structural ones of disciplines and curriculum. Without losing sight of my initial focus on the project of a feminist historical psychology and of transforming psychology, these concerns are touched on to broaden our thinking about this very project. In the end, my hope is that the reader will also indulge me the odd side road here and there, as I examine how the question of transforming psychology invites us to ask who or what becomes transformed and in which ways for what kind of Psychology and psychology.[3]

PSYCHOLOGY AND THE 'TURNS'—TO HISTORY, LANGUAGE AND CULTURE

In retrospect, Danziger's and my own paper were part of several decades of wider disciplinary encounters and shaping of what has variously been referred to as the historic, cultural or linguistic turn, or the turn to constructionism, feminism, science studies or critical science studies, or more broadly, the transition from modern to postmodern. By no means am I trying to collapse these various turns into one coherent framework, for they arose within different disciplinary contexts and, while informing one another, they nonetheless are singular neither in effect nor intent. Differing in their emphases, priorities, tools of analysis, and political commitments, these turns still share certain features of disciplinary and intellectual discontent that bear on more "general issues—philosophical, theoretical, ideological and methodological" (White, 1999, p. 315; also see Iggers, 1997). For psychology,

as for other disciplines, one source of critique came from a general disenchantment with views of science (Danziger, 1994), including a growing recognition of what passed for a "*nonpolitical*, scientific attitude" as instead a masculinist, liberal, capitalist bias (Israel, 1979, p. 254; Harding & Hintikka, 1983; Herschberger, 1948; Weisstein, 1971; Keller, 1985). Following from this was a disaffection with universalizing notions, such as the idea of the subject's nature operating in possessive individualism as one formed outside cultural-social-historical constraints (Hayles, 1999). Deriving from the joint forces of advocates and activists of parallel civil rights movements (e.g., blacks, women and lesbians and gays) along with antiwar and welfare rights struggles, more interpretative approaches began to displace those holding what I will loosely refer to as of a more positivist bent. These various turns—literary, linguistic, cultural and historical—have been thought of as "plac[ing] both agency and history back on the agenda" (McDonald, 1996, p. 5). They also served to place front and center a historiography of the everyday, and of those marginalized and forgotten (Iggers, 1997). Seen by many as issuing from "historical events interact[ing] with historiography and theory" (McDonald, p. 5; also see Bonnell & Hunt, 1999), these emerging approaches often served simultaneously as the basis of critique of traditional approaches to knowledge production *and* the basis for building alternative ones. From this vantage point, Danziger's 1994 piece "Does the history of psychology have a future?" represents a marker of over, at minimum, a decade's efforts devoted to disciplinary transformations, and recalls, in different ways, Roger Smith's (1988) "Does the history of psychology have a subject?"; Graham Richards's (1987) "Of what is the history of psychology a history"; and, Laurel Furumoto's (1989) "The new history of psychology." Together, these pieces tackle the interchange between the history of psychology and the theory of history, between epistemology and ontology, and between the discipline Psychology and its subject matter psychology. For each there grew comprehension of the significance of gender for understanding science and the practices of psychology.

With over another decade behind us, it is a propitious time to inquire again into this historic turn in psychology, asking ourselves to what extent and in what ways Psychology and psychology evidence signs—if any—of awakening to this "historical self-consciousness." But more than this, the topic cries out for deeper examination of how we might construe transformation and marginalization, including the linking of one to the other. In thinking about what counts as transformation or marginalization, we need also consider what, in using such terms, we avoid talking about, hide, or make more palatable—for ourselves and others. Far from a progress report, then, my examination provides a critical revisiting of our hopes to transform Psychology and psychology through critical history.

Before I begin my examination, one overarching concern remains. And that is what I sense to be our, or at least my, dwindling optimism for what was envisioned as a historicization of Psychology and psychology. My fading hope brushes up against my continuing strong commitment to a feminist critical history for psychology, and my unwavering belief that Psychology and psychology without history is not simply bad science but rather a disingenuous—maybe even dangerous—discipline and disciplinary subject. Strong words, I know. But, I am not alone in my concern. Just recently, Graham Richards (2002) wrote that for all the "wealth of history of Psychology scholarship accumulated since around 1980...history of psychology remains academically marginalized" (p. 8). His statement becomes all the more striking given the absence of Psychology and psychology in recent books devoted to the historic and the cultural turn (e.g., Bonnell & Hunt, 1999; McDonald, 1996), and the discipline's ongoing rather minimal representation in volumes on feminist and critical scholarship. Gergen (2001) makes a similar case around the absence of psychology in postmodernist debate. Within the discipline, other telling gaps appear. Whereas a healthy number of departments in colleges and universities offer a course on history of psychology, the teaching of this course is often undertaken by those for whom history is, at best, a secondary interest and the course content rarely includes work from the newer histories (Fuchs & Viney, 2002; also see Richards). Specialization in history by faculty members and programmes offering a track in history and theory of psychology (undergraduate and graduate) are rare.[4] All of this becomes even more surprising when we realize that the discipline has never really cemented a core curriculum (Benjamin, 2001). We need therefore to attend as closely to the absence or minimal inclusion of cultural history scholarship inside as outside of our discipline. Without representation in the broader discussions of cultural history and changing intellectual tides, cultural historical psychology loses authorial voice inside and outside the discipline, in psychology's curriculum and within the wider culture. However much cultural history may have to offer on the place of Psychology in psychology, and more generally in the tendency toward a psychologization of cultural, social, and political life, our efforts will go unnoticed unless we create more inroads into these discussions.

So, we cultural historians find ourselves, much as feminists have, with a paradoxical success—we continue to work out how the new history offers new ways to think about what counts as the psychological along with what counts as human and who does the counting while undergraduate curricula and the vast majority of psychologists in the Western world carry on *sans* pause. To what might we attribute this gap? If the areas of feminist and critical historical scholarship are developing nicely, where are its practitioners or their work to be found? It is to these issues that I now turn, beginning with professional reflections from feminist science studies scholars to gain insight into who or what becomes transformed with moves to new critical historical frameworks.

PSYCHOLOGY AND CULTURAL HISTORY: WHOSE TRANSFORMATION? WHOSE KNOWLEDGE?

Subject of Or to Transformation?

Sandra Harding (1991) writes that in the 1970s feminists began to "bring to bear on theories and practices of science and technology the distinctive approaches that had been developing in the social sciences, the humanities, and, more generally, the women's movement" (p. 19). Reviewing feminist advances since these critiques, Harding recognizes development toward a more historicized appreciation of gender and of science at the center of questions of whose science and whose knowledge. In reading through a set of essays in the recently edited volume, *Feminist science studies: A new generation*, I was reminded anew of Harding's questions and their importance to discussion on transformation. In particular, I was struck by a set of writings collected under the heading of "(un)disciplined identities: forging knowledge across borders." Introducing these papers, Angela Ginoria (2001) announces, almost celebrates, a sense of splits, fractures, of transgressions accompanying feminist reworkings of science in her title "Proud to be an oxymoron! From schizophrenic to (un)disciplined practice." Ginoria, a social psychologist, describes her insider-outsider status as conveying at once her personal and professional "internal" sense. She writes: "I have continued this schizophrenic existence—personally, as a Puerto Rican woman in mainstream America, and professionally, as an interdisciplinary scholar working on the seemingly unrelated issues of violence and science" (p. 14). Drawing on the chapters that follow, Ginoria highlights the diverse self-characterizations offered by the seven contributors as "insider-outsider," "meandering river," "self-reflexive boundary-straddling," "dual citizen," "resident alien," "boundary-crossing," and "naturecultures." Ginoria reads these terms, as do many of the contributors, as "empowering" tools to use in "decoding the experience that my [her] first discipline [psychology] would have labeled as maladjusted at best and schizophrenic at worst" (p. 19). Picart (2001), another contributor, reports a similar thing in relating the advice she imparts to students on being a "scientist who turncoated to become a philosopher": "I speak . . . with quiet conviction of the humorous, painfully ambivalent and creatively survival-rich experiences, of being at the limen—and invite them [students] to join in the cautious destabilization and joyous reworking of these borders" (p. 46).

One is hard-pressed to escape the bittersweet irony at work here, especially when you take into consideration that the transgressive labours of these feminist scientists are undertaken, for the most part, not within their respective scientific disciplines but rather within Women's Studies or some other interdisciplinary academic location. Insiders who have left, or never been allowed to enter, their primary disciplines complicate the story of insider-outsider status as in and of itself transformative. Their labours' migration from disciplinary to interdisciplinary locations

raises questions about who or what is being transformed in primary disciplines by this new generation of feminist science studies scholars. Their stories suggest we need to follow scholars' migration patterns. Are critical histories of psychology being taught outside the discipline? Are interdisciplinary programs offering tracks in a cultural history psychology? Where does a cultural historical psychologist or psychology reside? These migrations also provide an initial clue into what may be considered the myth of transformation, much like the myths of origin and objectivity early revealed in feminist critiques (e.g., Keller, 1985; Samelson, 1974). Transformation (or new found freedom) may be the promise but at what costs and for whom? This new generation, while not identical with the positions held and border transgressions made by earlier ones, nonetheless makes one mindful—all too sharply and even painfully—of transformation as a work undertaken in perpetuity. It is a work of generations, of vigilance, responsibility and politics *and* a work of fortune—good and bad. In short, to make claims on the transformative powers of marginalization, we need to know the relevant history.

As another example, for well-known scholar Donna Haraway (2000) reflecting back to 1979 when she accepted the "dream job" of feminist theorist in the History of Consciousness program at the University of California, Santa Cruz, the position solved several problems. As she says, the position was a "dream job" in that it allowed her to "work on what [she] really wanted to do, keep doing the political work that really mattered to her, and write about animals" (p. 38). It contrasted sharply with her time at Johns Hopkins University, where they were not going to promote her to Associate Professor, and where, during her review, a colleague had recommended she "erase" two of her publications from her CV "because they were too political and embarrassed [her] colleagues" (p. 38). So, the very reasons Haraway was denied promotion in one institution were precisely the qualifications for her acceptance elsewhere. She relocated to the History of Consciousness program, which was being revitalized by Hayden White and James Clifford, key figures in the linguistic or discursive turn. However positive the move was, the fact remains that those envisioning new creative directions may be penalized, and that disciplines, in shoring up their boundary defenses, may restrict not only the intellectual life of individuals but the profession, its academic curriculum and its intellectual livelihood.

Even though I have come across a number of these sorts of accounts, my own reaction on writing about them here is to rush to assure readers, and myself, that, of course, these sorts of views are so yesterday and that today we are well beyond either giving or receiving this type of advice. But the fact of the matter is that these stories remain all too common, and are often accompanied by feminist and cultural history scholars being prompted to move academic homes in search of more hospitable intellectual climes. Notwithstanding my own experience, I have come to know many psychologists working in a department or program outside of psychology. Part pushed, part prodded and part drawn to another location, the

moves are never simply about choice. By this, I do not mean to convey *all* switches from one to another institution, department or program result from disciplinary wrangling and policing. Nor do I want to be heard as positing that residing outside of Psychology is wrong or bad for psychology or psychologists or, for that matter, that residing in psychology is *de facto* good or a measure of success. Nonetheless, one has to speculate whether, if the discipline of psychology (and others) was open to alternative approaches, to feminism and more interdisciplinary scholarship, many of us would be happily ensconced in our "home" disciplines. Were departments and programs more hospitable to cultural history psychology and to its development within curriculum and research, we might well have been positioned more centrally than Richards (2002) found in his review. On imagining our numbers and our varieties of scholarship, one can't help but wonder how the face of Psychology and psychology would appear today. And, on imagining this, one cannot help but conjecture how restrictions shortchange all of us in the end at so many levels—institutional, professional, intellectual, everyday life—by reducing the possibilities of scholarship and teaching that may make a real difference for all of our lives.[5]

In inquiring into where historians in Psychology and other disciplinary historians might make their home, Danziger (1992) posed the dilemma as follows: The disciplinary historian "can elect either to bow to the moral authority of the discipline and become an ornament to the discipline by historicizing that authority, or to produce critical history for an audience composed entirely of other critical historians" (p. 10). Recognizing the potential risks for graduate students of psychology and for less and more established psychologists who pursue disciplinary history, Danziger sought to locate a place where scientists' and critical historians' interests converged. But, as he found, herein lies the rub: the place of convergence is precisely that hotspot of contention about the nature of science, including scientific authority and moral commitment. In Danziger's words, "It is when that authority becomes questionable, when the professional community is divided in some profound way, that a critical disciplinary history has a significant contribution to make" (p. 19). But as found by scholars of international psychologies, feminism, and the new historiography whose inquiries aimed to alter Psychology's near monolithic authority (Danziger), challenges to the discipline, whether localized in departments or more broadly within the academy, do not always eventuate in change. These inquiries were imagined initially to be of transformation-inducing potential by foregrounding the "particularly intimate connection between the historicity of the subject matter and the history of conceptions about that subject matter" (Danziger, p. 21). Yet, this very recognition bore paradoxical effects for scholars. Even as they reconceived significant fields within psychology—a historial social psychology or feminist historical social psychology—scholars, having transformed their teaching and scholarship, sometimes found themselves either at odds with their colleagues or in homes outside the discipline, as was the case for Haraway and Ginoria.

At the risk of being read as trading off talk of possibilities for stories of doom and gloom, it seems to me that we cannot afford to overlook this question of where cultural historians, feminist and otherwise, are located. To ignore this question does a great disservice to cultural history, to its place in formal organizations, to academic positions and curriculum, to our students (undergraduate and graduate), and, given the authority granted to psychology within North America, to culture more widely. Any question of how cultural history in psychology has served to remake Psychology and psychology must therefore bring within its purview the welfare of critical scholars, and of how we might more actively ensure their—our—livelihood. Likewise, we need to think about structural supports—about how to build and support cultural history programmes in psychology. In renewing our cultural history project, we might do well to adopt Haraway's (1997) attitude toward her work, an orientation she describes of her writing in general as "anxious much more than it is optimistic" (p. 44). For all of us, being "troubled or uneasy" (OED) about the place of cultural history in psychology may prove to be far more productive than undue optimism.

Having looked at *whose* transformation, the next step is to consider *what* becomes transformed—the subject of our disciplines, disciplines themselves, and/or the relation of our disciplines to other disciplines and programmes. To do so means extending this inquiry to cultural history psychology's larger context of emergence as followed through an historicized appreciation of area studies and interdisciplinarity, sites to which psychologists committed to cultural history and feminism sometimes move. Following this, we need to direct ourselves to the place of Psychology and psychology within, as Richards (2002) puts it, "the 'psychological ecology,' as it were, of modernist cultures" (p. 18). This means understanding how psychology "operates in contemporary culture" and the "matrix" of loosely, integrated elements in this production (Richards). I turn now to that historical tide of intellectual, social and cultural change most often earmarked as the transition from modern to postmodern as it was shaped by events from post-World War II on.

Subject of or to Knowledge?

At base in this inquiry lies a root conundrum for me: How could psychology as a discipline whose penchant claims to lie with evidence overlook scads of it in what appears to be an effort to secure a particular positivist narrative about its scientific nature and status? What transpires in psychology's longstanding love affair with this view of science to prevent the discipline from looking closely and deeply into the soul of its beloved? How are we to understand the seeming contradiction between the discipline's scientific commitments and its epistemological desires? Given the vast and longer history of critique on these very fronts, how might we approach psychology's entanglements and resistance to a cultural

historical psychology? Two avenues of exploration seem to offer ways for cultural historians to open up the conceptual deadlock created by repeatedly pitting a stable mainstream psychology against proliferation of alternative but subordinate approaches. One is to produce a more cultural historical analysis of this very relation between psychology's weddedness to a single version of science and scientific methods and cultural history's to more interpretive approaches. A second is to historicize relations between disciplines and interdisciplines. To pursue the former, I will look at the culture of disciplines and disciplinary relations through their signs and symptoms, as Marjorie Garber develops this analysis. Delivered with a bit of tongue-in-cheek, Garber posits a more (psycho)dynamic entanglement of desires in disciplinary relations in her effort to reconceive academic feuds and attacks on the academy. Despite our usual reluctance to engage Freudian and, for some, Foucaultian, analysis, Garber makes this pursuit worth the indulgence.

Signs and Symptoms of Disciplinary Affairs

"Disciplines," writes Garber (2001), "are constituted on the site of their own lack." In fact, she adds, "they are . . . twice so constituted" (p. 89). They are so, first, because "their desire is for genius," an impossibility as genius is neither structured nor rule bound whereas disciplines often operate with both. They are so, second, because "their existence is bound up with the continual attempt to coincide with [an] ideal [of themselves]" (p. 90)—an ideal that harbours a masculine sense for, as Atwood (2002) reminds us, women "never had a lot of Genius medals stuck onto them" (p. 100). The space between "the attempt and the idealization, the space of disciplinary desire," continues Garber, "is what we call 'theory'" (p. 90). Never really stabilized, except where theory becomes doctrine, desire is understood to be rather mobile. Disciplinary desire thus moves about as disciplines seek to realize their "ideal" self, making envy a discipline's "indwelling spirit" (p. 90). Garber's interest is thus with how envy makes disciplines tick.

Granted, in writing this, Garber premises "discipline envy" on its operating as "a mechanism—a structure. . . . New disciplines develop; others fade away. Envy, or desire, or emulation, the fantasy of becoming that more complete other thing, is what repeats" (p. 67). Playing on Freud's psychosexual dynamic of "penis envy," Garber sees a cultural proliferation of envy from cosmetic surgery (venus envy), journalists-turned-authors ("pencil envy"), and American's "magnitude envy" (big, bigger, biggest), through to "disciplinary—and literary—desires." Quoting psychologist Howard Gardner, Garber enters psychology's physics envy—its wish by aping the methods of physical science to *be* a science—as a case in point. The envy tag, however, has also been attached to those whose work fits more with one or another of the 'turns,' as, for example, recent indictments against science studies scholars. Often dubbed the 'science wars,' science studies scholars have been characterized as suffering from "science envy," a diagnosis turning their view of

science as culture, or as social relations and practices, into the sign and symptom of their envy.[6] Deemed envious of "science's privileged position," these scholars are regarded as seeking the privilege through the means of "exploiting that prestige" to enhance the "rigor" of their own critical discourse.

One simply cannot help but see a parallel to Freud's view of women's lack as translating into envy leading them to search (interminably) for the unobtainable. Their only hope is to approximate obtaining the desired object indirectly through a male child (p. 71)—their own power, status and moral authority therefore always in question and enacted by mediation through another. Through the gendered associations made of disciplines as either "hard" or "dry" (quantitative) or "soft" or "wet" (narrative, interpretive, descriptive) sciences, the *envy* idea circulates, rendering science studies scholars effeminate, as the ones subject to fantasies of completing their own taken-for-granted incompleteness through their critical relation to the object 'science' and without authority of their own. In Psychology this diagnosis is delivered without any sense of irony about Psychology's physics envy.

Much as Freud's notion of penis envy was about generations and symptoms of power so discipline envy can be understood as about lineage and indications of power. If, as Garber writes, "in current psychological shorthand, *envy* means not having it all: feeling a sense of loss, or limit, or even . . . nostalgia for a past imagined as more perfect and more whole" (p. 69), then we might ask of what else this sense is productive. We might further imagine "envy" as culturally productive for disciplines, even as it might be damaging for individual scholars. That is, envy is simultaneously about the possibility of sublimation and the risk of neurosis. It circulates in the space of disciplinary and interdisciplinary desire, the space within which idealized masculinity, neurosis and femininity arise as well as the space of culture, the space of both symptom and symbol. It creates both mainstream and alternative, and traps them in a dance of mirrorings.

From this perspective, we can imagine interdisciplinary programs or studies, as area studies (e.g., Asian, Women's, Medieval), as emerging or being egged on, in a manner of speaking, by disciplines. Interdisciplinary programs, therefore, function to "stage encounters" among disciplines, often through imagining new combinations, as, for example, science and literature or culture and science (Garber). The lost object, if not necessarily found, is promised to be fashioned anew. But if disciplines are founded on a site of their own lack, and if interdisciplines are the space of imagined potential wholeness, then, we need to look at relational dynamics between the two as a struggle to keep at bay anxieties about inevitable incompleteness. Framed this way, both appear doomed to a repetition of the fantasy of the other's basic *lack*—disciplines locating it in interdisciplinarity and vice versa. Their reflexive relation resembles Narcissus's to his own reflection, rather than a critical reflexivity that would enable moves beyond this repetitious pattern—we gaze at ourselves gazing at reflections of ourselves gazing back at our

representations *ad infinitum*. Haraway (1997) writes of something akin to this as a "relentless insistence on reflexivity, which seems not to be able to get beyond self-vision as the cure for self-invisibility" (p. 33). One result is the production and reproduction of the same thing, rather than breaking up the pattern to allow for something different (i.e., transformation). In these ways, envisioning transformation of disciplines as following automatically from more interdisciplinary scholarship may itself be a fantasy subject to ill-fated ends, especially if this dynamic enables disciplines to expel, deauthorize or declare alternatives as lying outside the discipline's intellectual mandate.

To expand further on these dynamic contexts of knowledge's emergence, Garber uses Foucault's (1977) recognition of "the disciplines mark[ing] the moment when the reversal of the political axes of individualization—as one might call it—takes place . . . that moment when the sciences of man became possible is the moment when a new technology of power and a new political anatomy of the body were implemented" (pp. 192,193). According to Foucault, and as used by Garber, "all the sciences, analyses of practices employing the root 'psycho-' have their origin in this historical reversal of the procedures of individualization" (p. 193). This process of individualization psychically charges disciplinary disputes and dissent and impels the narrative of nostalgia, a longing for a lost (imagined) "wholer version of oneself" or one's discipline or of knowledge itself (Garber, p. 89). This charge might be especially potent for those in psychology whose narrative has been overly determined by the discipline's struggle with its scientific status—its physics envy. This preoccupation has manifested in North American Psychology's over emphasis on methods to the detriment of its subject matter (Smith, 1997), and, perhaps, in the schism of APS from APA. Much as Freud was unable to critique idealized masculinity, so we might find disciplines and science, and, for that matter, interdisciplinary programmes and studies, unable to sustain or allow for a thoroughgoing critique of the idealized masculinity operating within their desires for a unitary perfected knowledge/self (perfect meaning complete, wholeness).

From a different angle, there is another way to think about these 'lacks' and their intellectual entanglements. What is missing in Psychology for cultural historians is a Psychology and psychology situated in time, place and circumstance. Many would agree that the hoped for change lies with methods attuned to more local contexts, or the everyday, with an appreciation of science as a social and cultural practice, as interpretive, and objectivity as a more critically reflexive position, situated in social relations and practices. Psychology's refusals and its asserting of universal insights into our nature outside the bounds of history and time, as Garber (1998) writes elsewhere, can also be read as symptomatic. That is, symptoms of the discipline's desire to free itself of its apparatuses of knowledge production, of those very social relations of practices that critical historians, feminists, and science studies scholars have been detailing in developing other ways to produce knowledge. Danziger (1994) writes of something close to this in what he calls,

after Max Weber's "disenchantment of the world," the "disenchantment of science" (p. 473). According to Danziger, Weber was referring to "a historical process in which science played a major role. In this process the world ceased to be an arena for miracles and spirits and for divinely inspired moral dramas and became an arena for human calculation and rational prediction" (p. 473). Art and science parted ways. Foucault's (1977) notion of the "psycho-" technologies of rational measurement and management—a "calculable man" (p. 193) make evident complaints captured by our current "disenchantment of science".

Using the case of the "culture wars," Garber instances how this 'war' tells a larger story of "striking symptoms of culture in our time." For Garber, the "culture wars" reflect a "conflict that might be located precisely in the clash between the timeless, ahistorical, universalizing, decontextualizing function of the 'symbol' and the historically contingent, specific, and overdetermined function of the 'symptom'" (p. 7). While Garber then looks at the implications of this for literature, her analysis can be extended to psychology to read: Psychology as "symbol" is "expected to proclaim 'timeless, universal, truths'; [psychology] as 'symptom' is embedded in particular historical preoccupations and conflicts, both in its own time and in ours" (p. 7). Garber's analysis is edifying in that she helps us to connect the dots between the problems for *psychology* of a *Psychology* that is symptomatic of "decontextualization, historical forgetting" and the "erasure of conflicting forces." For one, the struggle between the local and the global, or specific and general, identifies one line of debate between cultural historical psychologists and positivist science paradigms. For another, there is operating in this cultural symptom a "tyranny of the empirical" that Garber regards as impoverishing our cultural understandings. In her words, "our culture likes numbers, statistics, 'facts.' As if a fact were somehow the end of the story rather than the beginning" (p. 6). What this empirical point of view *lacks*, are ways to read the cultural, and to locate or create meaning. To really bring the risk of a calculable subject home, Garber invites us to imagine what would have happened to the Oedipal drama had it been wrought through hypothetical exploration of "the statistical occurrence of dysfunctional families within a) 'real' Theban households of the period, or b) classical tragedy. How many royal babies were exposed on hillsides? What were the social and economic pressures compelling the remarriage of widows?" (p. 5). No need to belabour the point.

I have found Garber's turn to psychoanalysis in both her thinking through of discipline envy and of the symptoms of disciplinary cultures useful for how effectively she redirects attention to the arena of desires, fantasies, and imaginings. Her work prompts us to delve further into psychology's place in creating and sustaining this "calculable subject," or culture of the "empirical self" (Bayer, 2002). Indeed, this form of a rational subject underlies much of Psychology's approach to the psychological, of which cultural historical psychology has provided astute analysis upon analysis. Perhaps many of us assumed that by showing

how Psychology and psychology have themselves grown out of and been involved in shaping cultural history, that the discipline itself would change (or convert) its knowledge-making practices. But the dynamics, the networks, and the cultural entrenchment of Psychology and its hold on psychology run deep, and those of us working in the critical historical vein have perhaps been ourselves less wide-ranging or outside the ordinary than we might have thought. In fact, we have in many of our critical engagements with Psychology's paradigmatic, theoretical or historical lacks, however wittingly or unwittingly, been given to certain repetitions ourselves. At various times we have identified how our approaches might fill a gap, provide the connective tissue in a narrative of disciplinary progress (despite our critique of grand narratives), or assumed some democratic (rational) model of representations of constituencies as bringing about a more diverse discipline (Bayer, 1995, 1997; Danziger, 1994; Richards, 1987, 2002). We may also have been far too quick to supplant notions of a calculating subject with those of a regulating, disciplining (Foucaultian) one. We have undoubtedly directed our efforts too inwardly, too much at the discipline itself and perhaps not enough toward interdisciplinary sites of critical historical work, at creating a space for cultural historical psychology in science studies, women's studies, cultural studies, history of science and so on.

Looking to Wider Contexts of Emergence

Approaches in critical cultural historical psychology could be said to have emerged from that larger context of a massive and far-reaching historical transition from modern to postmodern, which we are still undergoing and which is often characterized as a changeover from an industrial-based to information-based economy (see Hayles, 1999). Its momentum in the United States has put it on a scale akin to the "turmoil at the beginning of the Industrial Revolution in England" (Traweek, 2000, p. 22). Pursuing this political, social, intellectual and cultural "revolution" from the period of World War II, Traweek traces how this upheaval brought with it demands for new kinds of knowledge and knowledge production by "new kinds of researchers at new kinds of universities" running all the way from "new defences against new weapons" through to "new ways for government to communicate with citizens" (p. 38; also see Herman, 1995). Psychology was among those disciplines most affected, along with the physical sciences, engineering, economics, and political science, creating interdisciplinary sites for research (Traweek). Interdisciplinary think tanks, such as the cybernetic conferences, and research centers cropping up around group dynamic research were part of this shift-change (Edwards, 1996; Herman, 1995). To quote Traweek, "cold war sensibilities" encouraged "mission-oriented" research to protracted effect, and psychology was no exception.

The term 'studies' signaled interdisciplines, "nexes of overlapping interest," and reflected in its postwar coinage, a sign of the "increasing interest in

non-European or non-Western regions of the world" (Garber, 2001, p. 77). From the 1950s on, interdisciplinary and area studies grew in number and kind, often being refitted to the times in their changing emphases from temporal to geographic to cultural markers. Most of us readily refer to the sixties and seventies as ushering in ethnic, sexuality and women's studies. Traweek sees temporal centers as characteristic of new area studies in the 1980s, followed by cultural studies, environmental studies, and we might now add postcolonial and global studies. Traweek notes that "at UCLA the list of interdisciplinary research 'centers' is longer than the list of the traditional disciplinary departments, as is true of most highly ranked American research universities" (p. 45). Despite how these seemingly "postdisciplinary" sites have grown since the 1950s and come to characterize or to be used as an 'unique' benchmark of institutions, as at the liberal arts institution where I teach, there remains a stubborn hold by disciplines on "the definition of intellectual authority" (Traweek). If my institution offers any insight, this contradictory arrangement has to do with a failure on the part of administration to provide structural support outside of disciplines. These contradictions may prove damaging to institutions, to faculty and to students who allow disciplines to promulgate misinformation that jobs are few and far between for those holding interdisciplinary degrees or adopting interdisciplinary approaches (Traweek; Balsamo, 2000a). Differences in intellectual authority can also result in academic wrangling over department affiliation, the status of courses and scholarship, the relation of interdisciplinary or area studies or centers to disciplines, and so on. Added into this complex of power relations is the backlash against ethnic and minority studies of late, making for a rather chilly climate for many (Balsamo, 2000b).

To overlook this context, is to ignore, on the one hand, the very basis of undertaking critical history, feminist and otherwise. On the other hand, it also makes us shortsighted about the ways in which departments and disciplines continue to exercise their authority despite mounting pressure for change, as Danziger observed. So, we need to situate historically disciplines, interdisciplines and area studies in trying to understand transformations-in-the-making, their promises and the resistance they meet. Without this, we may misdirect our struggle for recognition from the discipline and in psychology curricula in ways that continue to limit real possibilities for change. Failing to address our context of emergence and a much more complex, rich and varied history of science and practices of knowledge within psychology also risks continued solidifying and reifying of a disciplinary authority that has little basis in Psychology's own history of methods, practices or struggles with its subject. We simply can no longer afford to succumb to barren counter-challenges to our work, such as those boiling down to some equation of declining interest in positivism with either the demise of the discipline itself or our own legitimacy as psychologists. Such equations illumine psychology's cultural symptoms and worries as about its status as a science and its wider cultural currency. They also reveal the discipline's tendency to misidentify the ideology of its

(masculine) power as a "natural" instead of historically rooted authority. What this standard criticism cannot provide is a form of engagement with changing practices of knowledge production or changing notions of our subject, or indeed emerging interdisciplinary sites of psychological inquiry.

FROM MYTHS TO PRACTICES OF TRANSFORMATION

To situate psychology within the transition from modern to postmodern would be to resituate various streams of work as more central than typically recognized. Among these would be cultural historical psychology, including the growth of centers of study marked by a particular approach, such as narrative, discursive or critical psychology, the long history of feminism in psychology, as well as "mission-oriented" research centers, civil rights work, and cybernetics along with other political economic-based shifts. The history of the discipline would also be substantively transformed were it told through the framework of struggles over methods, alternative approaches and so on. Tracing this story through works following the various 'turns' would be an important component of a critical examination of our quest to transform Psychology and psychology. With but a moment's pause, a number of relevant works flock to mind, including Buss's (1979) *Psychology in social context*; Dinnerstein's (1976) *The mermaid and the minotaur*; C. Sherif's (1976) *Orientation in social psychology*; Weisstein's (1971) "Psychology constructs the female;" Samelson's (1978) "From 'race psychology' to 'studies in prejudice'"; Henriques et al.'s (1984) *Changing the subject*; Kitzinger's (1987) *The social construction of lesbianism*; Morawski's (1988) *The rise of experimentation in psychology*; and, Danziger's (1990) *Constructing the subject*; and, more recently, Graham Richards's (1997) *'Race', racism and psychology: Towards a reflexive history* and Katherine Pandora's (1997) *Rebels within the ranks*. Even from this limited selection, one can gain a sense of the sorting out, in recent decades, of questions that derive from psychology being a part of larger social and cultural historical changes and from debate on the production of knowledge for Psychology and psychology. These works not only supply relevant lines for us to historicize the project of a cultural history psychology but they serve to underscore the need for critical historical scholarship to become as mindful of its own gaps and shortcomings as it is about the discipline's more generally.

Here's what I mean by this. Within critical science studies, Haraway (1997) observed science studies scholars' analyses as limited by their failure to draw from the "understandings of semiotics, visual culture, and narrative practice coming specifically from feminist, postcolonial, and multicultural oppositional theory" (p. 35; also see Golinski, 1998). Offering critical reads of science, some scholars nonetheless rendered their analyses through narratives of heroic action, or stories of "trials and feats of strength, amassing of allies, forging of worlds in the

strength and numbers of forced allies" (p. 34). Some accounts have also "mistaken other narratives of action about scientific knowledge production as functionalist accounts appealing in the tired old way to preformed categories of the social, such as gender, race, and class" (p. 35). As she puts it, "either critical scholars in antiracist, feminist cultural studies of science and technology have not been clear enough about racial formation, gender-in-the-making, the forging of class, and the discursive production of sexuality *through the constitutive practices of technoscience production themselves*, or the science studies scholars aren't reading or listening—or both" (p. 35). A similar argument could be made of critical historical and critical feminist scholars in psychology for we too fail to engage regularly one another's work, devoting more attention to the faultlines between cultural history and the mainstream of psychology and making for shortsightedness.

For critical history scholars to engage with one another's work is to allow cultural historical psychology to enlarge its work on science, practices of knowledge, and notions of the subject. Paralleling Haraway's (1997) finding of new understandings of masculinity and scientific observers by reexamining science studies histories of early modern scientist Boyle's laboratory, we might engage ground breaking works in cultural history psychology, such as Danziger's (1990) on social relations and practices in Psychology's "constructing the subject." For example, in looking at the models of education and testing, clinical practice and hypnosis, and introspection, we might inquire into who was being made in what counted as credible witnesses or observers, and as credible subjects of science. Surely gender was at much at stake in constructing the subject of testing as it was in introspective observers and in clinical subjects. What would this critical history add to our understanding of psychology's participation in gendering authority, ways of life and practices of science? And what types of masculinity were at stake in forging the rational, testable and calculable subject of psychology? Likewise we might inquire into how our discipline's history would look if told through female subjects' performances in spaces signified by the couch, the laboratory, the clinic, and the field where they facilitated the interchange of meanings between the world outside and the world inside the discipline without authority in either. What I am arguing for is sustained treatment of gender-in-the-making in which gender "is the relation between variously constituted categories of men and women (and variously arrayed tropes), differentiated by nation, generation, class, lineage, color, and much else" (Haraway, p. 28).

Critical histories attending to these complex relations constituted *in* the making of psychological knowledge are sorely needed and are most assuredly not of significance only for women and people of color. Building on our work rather than reiterating our divergence from the mainstream is crucial. To assume otherwise is to repeat the very timeworn and thinly veiled dualism invoked by mainstream scholars of work on gender, race, class, sexuality and so on as about "special interest groups"—ideology—rather than serious scholarship. One would have thought

this criticism to have outlived its day long ago, especially given innumerable demonstrations of the very ideologically-laden premises and parameters of logical positivist science (e.g., Haraway). But, as Steinberg (1996) discovered when a new faculty member, the term "ideologue" continues to be invoked tactically to pressure faculty to follow certain departmental tradition or "scholarly" ways. On reflection, Steinberg says that he saw his "epistemological and ethical choice not as one between tradition and innovation or between objective and ideological scholarship, but between the tradition of ideologically marked 'normal' science and the tradition of critical thinking and, most specifically, of the critique of ideology" (p. 105). He was thus, "loath to accept the scholar/ideologue distinction because of its kneejerk acceptance of the myth of objectivity" (p. 105). His story is instructive for how he helps us to move beyond the myth of transformation to practices of transformation.

Engaging a fuller range of our cultural historical psychology work will also serve to build upon already strong scholarship on epistemology, and ontology, most especially on what Smith (1997) calls the "core problem" of subject-object relations. At heart, for Smith, are such questions as "how, objectively, can we observe ourselves? By the process of observation, do we not make ourselves into something different?" (p. 15). Without recounting it in detail, there is a healthy body of scholarship on the problem of objectivity, whether approached as characteristics of the observer, scientific practices or paradigmatic assumptions. Such critiques freed us from the view that objectivity—whether of the observer or scientific practices—was neatly achieved by assumptions of detachment or 'hands-off' methods (e.g., Haraway, 1997; Keller, 1992; Megill, 1991; Nagel, 1986). A more reflexive appreciation was called for. Philosopher Elizabeth Grosz (1993) pinpointed the problematic well: If critical inquiry into the subject of knowledge has located a blind spot in knowledge production of reason's inability to "know the knower," then a "discipline whose object is *man* is necessarily incomplete unless it can include its own production as a discipline within the knowledge it produces" (p. 11). For those adhering to positivist formulations of a scientific psychology, this reflexive relation has wreaked havoc, calling forth endless defensive maneuvers in an effort to shore up the line between those scientific psychologists doing the observing and their objects of observation—humans, animals, birds, fish, computer programs and so on. Interestingly, the more efforts are directed in this way, the more they reveal the very constitutive nature of subject-object relations (e.g., Morawski, 1992; Rosenzweig, 1933).

That observers cannot get around this reflexive relation is probably more readily agreed upon than how this reflexive relation functions or might function in the production of scientific knowledge. One thing we do know is that the historicity of reflexivity remains relatively unexamined for its changing meanings across different 'turns'. Hayles (1999) traces one significant shift in subject-object relations, and the meaning of reflexivity to post-WorldWar II social movements

and the emerging area of cybernetics (also see Haraway). Cybernetics, she argues, carried potent new ways to think about feedback in terms of circuits of information that flowed through us. Soon these ideas began to subvert versions of "observers as outside the system they observe" such that information could loop "*through* the observers, drawing them in to become part of the system being observed" (p. 9). This shift entailed a wholehearted makeover from more static, even if constructionist, notions to more constitutive notions of relations between observers and observed, and between both of these and the broader cultural context (e.g., Abbott, 2001).

Cybernetic influences reworked visions of the human as a set of "informational processes" whose erasure of our embodied ways of being set itself apart from "other critiques of the liberal humanist subject, especially in feminist postcolonial theories" (Hayles, 1999, p. 4; also see Edwards, 1996). It did so mainly by positing a relational flow of information as the key organizing idea to the liberal humanist subject, displacing previous notions organized around possessive individualism in which the body marked the boundary of the self and was conceived of as the container of our 'selves,' even though this rational subject was "not usually represented as *being* a body" (p. 4, author's emphasis; also see Bordo, 1993). Of course, one might usefully argue that cybernetics ushered in a particular kind of embodied subject organized through information, codes and so on but key for us here is the idea of the constitutive nature of subject-object relations.

These more constitutive approaches to knowledge-making relations and practices have led over past decades, as I noted earlier, from ways to critically examine to ways to critically re-envision how we might proceed in psychology. Insofar as "linguistic and symbolic discourse is primary in our world, then the history of discourse, the history of the way cultures picture themselves to themselves, is central to the human sciences. In other words, from this perspective the history of the human sciences is itself a human science" (Smith, 1997, p. 870). That is, the quest to think about whose science and whose knowledge has led not only to changing appreciation of the relation between Psychology and psychology, but how science, culture, politics, language, and much more are part and parcel of any cultural historical psychology.

By zeroing in on local cultural histories, the project of a cultural historical psychology enlarged approaches to the study of psychology in everyday life to considering as well the discipline's hand in fashioning a modernist subject, and in translating so much of life, politics and culture into psychological turns. Indeed, "the psychological" has been characterized as a "great modern ideological system" of the twentieth century (Williams, cited in Pfister, 1999a). By the 1920s and 1930s, Pfister (1999b) observes, the field of psychology had a "growing popularity and even the cachet of 'psychological' identities partly produced by and made available through mass and high culture (psychological and pop psychological books, articles, and advertisements, therapy, literature, theater, films, art)"

(p. 167). In fact this "psychological" spin on subjectivity, says Pfister, became, in some ways, a "hallmark of the 'modern'" (p. 167). Treating "the psychological" as Foucault did "sexuality", Pfister's more critical history approach allows for an examination of "the psychological" as a "popular 'truth' discourse freighted with meanings, values, significances, and practices" (p. 169). His cultural history is instrumental, and offers ways to think more contextually and historically about Psychology's cultural ecology with emerging psychologies of everyday life. It thereby extends earlier works looking at Psychology's part in our changing psychology, whether by ushering in new ways to think about ourselves as psychological subjects, as in Freudian, behaviorist, or sociobiological terms, or by characterizations of psychological phenomena (e.g., MacIntyre, 1985; Richards, 2002). Cultural histories of this sort extend beyond the value of evidencing notions of the subject and subjectivity in flux to a discipline whose history evidences all the signs and symptoms of a similarly changeable 'nature.' What awaits us is making this known.

TRANSFORMATIONS—FOR THE LOVE OF THE DISCIPLINE

Those of us who entered psychology in the years of the 'turns' probably felt the winds of change to be stronger than we do now in hindsight. This awareness, however, need not dampen our enthusiasm for a cultural history psychology. If anything, social and historical events of late combined with what many deem a conservative backlash of considerable force makes our project all the more pressing. That is, since the attack on the World Trade Centers in the U.S. on September 11, 2001, there is a renewed and profound emphasis on what we do not understand in our increasing globalized lives, of how limited our understandings of culture, self and other have been, and of how quickly a nation, such as the U.S., can render dissent of views as a threat to its sense of security. Much as the post-World War II protests and movements registered piercing questions into rights, of identity and politics around gender, sexuality and race, and of a renewed appreciation of the limits of scientific objectivity, so globalization along with changing technologies has set in motion inquiries into the cultural and political economies of our daily lives. In the academy, interdisciplinary programs are in the midst of regenerated interest. Cultural historical psychology is of a different age now too, and perhaps more ready than before to assert its voice in areas that one would have expected Psychology to have a say in—subjectivities, social psychology of group relations, and identities formed through contexts of competing histories, beliefs, cultures, and politics. To do so, we will need to turn to practices of transformation that call on cultural history psychologists of every ilk far and wide for the livelihood of cultural historians in psychology is indeed about ensuring we live in worlds, (inter)disciplines, departments and curricula that matter.

NOTES

[1] Thanks to Adrian Brock for his support in the preparation of this chapter. I am deeply grateful to Susan Henking whose generous and scholarly advice helped to shape ideas for this chapter.

[2] This title is taken from Snitow's (1989) on the feminist divides that have arisen around interrogation of 'woman'.

[3] I follow here Graham Richard's (1987) convention of designating the discipline, Psychology, by uppercase, and the subject, psychology, using lowercase. I am also borrowing loosely from Haraway's (1997) discussion on science's experimental ways of life as that space in which "what will count as nature and as matters of fact get constituted for—and by—many millions of people" (p. 50).

[4] The APA web site does not itself promote history of psychology. In the United States as in Canada, there appears to be but one program offering graduate work in history, in which students are encouraged to develop a dual focus—one in a traditional (experimental) field and one in history.

[5] Laurel Furumoto (1988) wondered a similar thing in her work on the exclusion of women from Titchener's elite society, The Experimentalists.

[6] The most publicized case of this was the hoax created by physicist Alan Sokal.

REFERENCES

Abbott, A. (2001). *Chaos of disciplines*. Chicago: University of Chicago Press.

Ashmore, M. (1989). *The reflexive thesis: Wrighting sociology of scientific knowledge*. Chicago: University of Chicago Press.

Atwood, Margaret (2002). *Negotiating with the dead: A writer on writing*. Cambridge, UK: Cambridge University Press.

Balsamo, A. (2000a). Engineering cultural studies: The postdisciplinary adventures of mindplayers, fools and others. In D. Reid & S. Traweek (Eds.), *Doing science + culture* (pp. 259–274). New York: Routledge.

Balsamo, A. (2000b). Teaching in the belly of the beast: Feminism in the best of all places. In J. Marchessault & K. Sawchuk (Eds.), *Wild science: Reading feminism, medicine and the media* (pp. 185–214). New York: Routledge.

Bayer, B. M. (1995). Life part way in, part way out. *History and Philosophy of Psychology Bulletin, 7*, 36–40.

Bayer, B. M. (1997). Reenchanting constructionist inquiries. In B. M. Bayer & J. Shotter (Eds), *Reconstructing the psychological subject* (pp. 1–20). London: Sage.

Bayer, B. M. (2002). Critical contact: Psychology, the subject, and subjectivity. *Feminism & psychology, 12(4)*, 455–461.

Benjamin, L. T. Jr. (2001). American psychology's struggles with its curriculum: Should a thousand flowers bloom? *American Psychologist, 56*, 735–742.

Bonnell, V. E. & Hunt, L. (Eds). (1999). *Beyond the cultural turn*. Berkeley, CA: University of California Press.

Bordo, S. (1993). *Unbearable weight: Feminism, western culture, and the body*. Berkeley, CA: University of California Press.

Buss, A. (Ed.). (1979). *Psychology in social context*. New York: Irvington Publishers, Inc.

Danziger, K. (1990). *Constructing the subject: Historical origins of psychological research*. New York: Cambridge University Press.

Danziger, K. (1992). In praise of marginality. Paper presented at the meeting of Cheiron-Europe, Groningen University, Netherlands.

Danziger, K. (1994). Does the history of psychology have a future? *Theory and Psychology, 4*(4), 467–484.

Dinnerstein, D. (1976*). The mermaid and the minotaur: Sexual arrangements and human malaise*. New York: Harper & Row.

Edwards, P. N. (1996). *The closed world: Computers and the politics of discourse in cold war America*. Cambridge: The MIT Press.

Foucault, M. (1977). *Discipline and punish*. New York: Vintage Books.

Fuchs, A. H. & Viney, W. (2002). The course in the history of psychology: Present status and future concerns. *History of Psychology, 5*, 3–15.

Furumoto, L. (1988). Shared knowledge: The experimentalists, 1904–1929. In J. G. Morawski (Ed.), *The rise of experimentation in psychology*. New Haven, CT: Yale University Press.

Furumoto, L. (1989). The new history of psychology. In I. Cohen (Ed.), The G. Stanley Hall Lecture Series. (Vol. 9). Washington, D. C.: American Psychological Association.

Garber, M. (1998). *Symptoms of culture*. New York: Routledge.

Garber, M. (2001). *Academic instincts*. Princeton: Princeton University Press.

Gergen, K. (2001). Psychological science in a postmodern context. *American Psychologist, 56*, 803–813.

Ginoria, A. B. (2001). Proud to be an oxymoron! From schizophrenic to (un)disciplined practice. In M. Mayberry, B. Subramaniam & L. W. Weasel (Eds.), *Feminist science studies: A new generation* (pp. 14–21). New York: Routledge.

Golinski, J. (1998). *Making natural knowledge: Constructivism and the history of science*. Cambridge: Cambridge University Press.

Grosz, E. (1993). Bodies and knowledges: feminism and the crisis of reason. In L. Alcoff & E. Potter (Eds.), *Feminist epistemologies* (pp. 187–215). New York: Routledge.

Haraway, D. J. (1997). *Modest_witness@second_millennium.* New York : Routledge.

Haraway, D. J. (2000). *How like a leaf: An interview with Thyrza Nichols Goodeve.* New York: Routledge.

Harding, S. & Hintikka, M. B. (Eds). (1983). *Discovering reality: Feminist perspectives on epistemology, metaphysics, methodology, and philosophy of science.* Boston: Reidel.

Harding, S. (1991). *Whose science? Whose knowledge? Thinking from women's lives*. Ithaca, NY: Cornell University Press.

Hayles, K. (1999). *How we became posthuman: Virtual bodies in cybernetics, literature and informatics.* Chicago: University of Chicago Press.

Henriques, J., H., W., Urwin, C., Venn, C. & Walkerdine, V. (1984/1998*). Changing the subject: Psychology, social regulation and subjectivity*. London: Routledge.

Herman, E. (1995). *The romance of American psychology: Political culture in the age of experts*. Berkeley, CA: University of California Press.

Herschberger, R. (1948). *Adam's rib*. New York: Pellegrini and Cudahy.

Iggers, Georg, G. (1997). *Historiography in the twentieth century*. Hanover, NH: Wesleyan University Press.

Israel, J. (1979). From level of aspiration to dissonance. In A. R. Buss (Ed.), *Psychology in social context*. New York: Irvington Publishers, Inc.

Keller, E. F. (1985). *Reflections on gender and science*. New Haven: Yale University Press.

Keller, E. F. (1992). The paradox of scientific subjectivity. *Annals of Scholarship, 9*, 135–53.

Kitzinger, C. (1987). *The social construction of lesbianism*. London: Sage.

MacIntyre, A. (1985). How psychology makes itself true—or false. In Sigmund Koch & David E. Leary (Eds), *A century of psychology as science* (pp. 897–903). New York: McGraw-Hill.

Mayberry, M., Subramaniam, B. & Weasel, L. W. (Eds.) (2001). *Feminist science studies: A new generation*. New York: Routledge.

McDonald, T. J. (1996). Introduction. In T. J. McDonald (Ed.), *The historic turn in the human sciences* (pp. 1–14). Ann Arbor: The University of Michigan Press.

Megill, A. (1991). Introduction: Four senses of objectivity. *Annals of Scholarship, 8*: 301–319.

Morawski, J. G. (1988). *The rise of experimentation in psychology*. New Haven, CT: Yale University Press.

Morawski, J. G. (1992). Self-regard and other-regard: Reflexive practices in American psychology, 1890–1940. *Science in Context, 5*, 281–308.

Nagel, T. (1986). *The view from nowhere*. Oxford: Oxford University Press.

Pandora, K. (1997). *Rebels within the ranks: Psychologists' critique of scientific authority and democratic realities in new deal America*. Cambridge, UK: Cambridge University Press.

Pfister, J. (1999a). On conceptualizing the cultural history of emotional and psychological life in America. In J. Pfister & N. Schnog (Eds), *Inventing the psychological* (pp. 17–59). New Haven: Yale University Press.

Pfister, J. (1999b). Glamorizing the psychological: The politics of the performances of modern psychological identities. In J. Pfister & N. Schnog (Eds), *Inventing the psychological* (pp. 167–213). New Haven, CT: Yale University Press.

Picart, C. J. S. (2001). Through the lens of an insider-outsider: Gender, race, and (self-) representation in science. In M. Mayberry, B. Subramaniam & L. W. Weasel (Eds.), *Feminist science studies: A new generation* (pp. 42–47). New York: Routledge.

Richards, G. (1987). Of what is history of psychology a history? *British Journal for the History of Science, 20*, 201–211.

Richards, G. (1997). *'Race', racism and psychology: Towards a reflexive history*. London: Routledge.

Richards, G. (2002). The psychology of psychology: A historically grounded sketch. *Theory & Psychology, 12(1)*, 7–36.

Rosenzweig, S. (1933). The Experimental Situation as a Psychological Problem. *Psychological Review, 40*, 337–354.

Samelson, F. (1974). History, origin myth and ideology: 'Discovery' of social psychology. *Journal for the Theory of Social Behavior 4*(2): 217–231.

Samelson, F. (1978). From 'race psychology' to 'studies in prejudice:' Some observations on the thematic reversal in (social) psychology. *Journal of the History of the Behavioral Sciences, 14*: 265–278.

Sherif, C. W. (1976). *Orientation in social psychology*. New York: Harper & Row.

Smith, R. (1988). Does the history of psychology have a subject? *History of the Human Sciences, 1*(2), 147–177.

Smith, R. (1997). *The Norton history of the human sciences*. New York: W. W. Norton & Company.

Snitow, A. (1989). A gender diary. In A. Harris & Y. King (Eds.), *Rocking the ship of state: Toward a feminist peace politics* (pp. 35–74). Boulder: Westview Press.

Steinberg, M. P. (1996). Cultural history and cultural studies. In C. Nelson & D. P. Gaonkar (Eds.), *Disciplinarity and dissent in cultural studies* (pp. 103–129). New York: Routledge.

Traweek, S. (2000). Faultlines. In D. Reid & S. Traweek (Eds.), *Doing science + culture* (pp. 21–48). New York: Routledge.

Weisstein, N. (1971). Psychology constructs the female. In V. Gornick & B. K. Moran (Eds.), *Women in sexist society: Studies in power and powerlessness* (pp. 207–224). NY: New American Library.

White, H. (1999). Afterword. In V. E. Bonnell & L. Hunt (Eds.), *Beyond the cultural turn* (pp. 315–324). Berkeley, CA: University of California Press.

WUNDT AS AN ACTIVITY/ PROCESS THEORIST

AN EVENT IN THE HISTORY OF PSYCHOLOGICAL THINKING

Hans van Rappard

INTRODUCTION

The first time I met Kurt Danziger was at the 1981 conference of Cheiron in River Falls, Wisconsin. One day, I found myself walking next to Kurt on the way to the campus restaurant for lunch. I frantically introspected for a striking line to start a conversation and finally came up with "I admire your work on Wundt". It worked.

About 15 years later, Wundt played a different role. In a paper on the future of the history of psychology Danziger (1994) noted that over the past decade professional historians had become increasingly salient in psychology. Until the 1980s the field had virtually been the exclusive domain of psychologists in the reflective phase of their life-cycles. Since a similar change from insider to outsider historian had occurred in the natural sciences too, Danziger questioned the relation between the two historiographies. In a comment I argued that the picture of the insider/scientist-scholar painted by Danziger was fairly bleak and called for a touch of colour (Van Rappard, 1997). Setting out to add some, I referred to a couple of lines in Danziger's paper where he observed that while Weber and Durkheim, and Smith and Ricardo are still studied by sociologists and economists, "Newtonian studies are not part of physics but belong to an altogether *different* discipline, the history of science" (Danziger, 1994, p. 472, emphasis added). These lines formed the *Blickpunkt* of my comment. Why mention sociologists and economists, I asked,

but not psychologists? Cannot it be easily observed that in our field classics like Wundt, James and Vygotsky frequently feature in the foundational, theoretical and/or critical studies that are still abundantly published in psychology? And I added that in such works, Wundt, James and Vygotsky (and many others) are not studied because of their purely historical interest but because of their paradigmatic views on the field. Pertaining to sociology Kuklick wrote, "the founders of the discipline—and particularly the so-called 'holy trinity', composed of Weber, Marx, and, above all, Durkheim—are regarded as *still-active participants* in sociological debates" (Kuklick, 1999, p. 232, emphasis added). I contend that this holds for psychology too. According to Zuriff (1985),

> "... the science of behavior has made significant strides over the past seventy years, and the behavioral scientists of 1920 have very little of interest to say to their counterparts of today concerning behavioral theory. However, on the conceptual level, behaviorism (and psychology in general) has not shown similar progress. The fundamental questions concerning the nature of psychology have not been answered, nor is it obvious that progress is being made. Instead, certain recurring themes are discussed, debated into a numbing stillness, and dropped unsettled, only to reappear years later in a different guise and under a new terminology. Therefore, the behaviorist of 1920 may have much of relevance to say to the modern psychologist formulating a conceptual framework for psychology. Indeed, early behaviorists often are more explicit on the reasons behind a particular position than later works, which may adhere to a behaviorist tenet out of a sense of loyalty to a school even though the original reasons have long been forgotten or may no longer be valid". (p. 5)

Just like in sociology and economics, contemporary psychologists often think it useful to dig out the conceptual veins that can be mined from the works of the classics in order to participate in current debates. It should be clear that I am referring to the debates on 'the fundamental questions concerning the nature of psychology' (Zuriff), which, although neglected by mainstream researchers take place among theoretically inclined psychologists (e.g., Barbalet, 1999; Greenwood, 1999; Harré, 2000). By their very nature such questions border on philosophy, where, according to Haldane (2000), "... there is hardly anything new under the sun—or in our thoughts about our ideas of it" (pp. 468–469). This explains, I think, the place of the classics in the theoretical debates in psychology, sociology, and economics as well as, of course, philosophy. In many cases the participants in these debates are not, and given their purpose need not be, concerned about the strictures of current historiography. After all, these 'still-active participants' most often are scientist-scholars who are interested in the classics of their field from an insider point of view, that is, they take a science-related perspective. As I see it, it is in this context that the contemporary relevance of history for psychology may be understood, along with the place of the insider historian. In this regard the distinction made in economics between the economic and social *history* of the field on the one hand, and the history of economi*c thinking* on the other, may be useful

to keep in mind. Kelley (2002, p. 12) has drawn an analogous distinction between cultural and intellectual history, whose relationship is described in terms of each other's 'outside' and 'inside', respectively. Fitting in with this suggestion, a practical difference between economic history and the history of economic thinking is that economic history tends to be taught by professional historians—economic outsiders in other words—but the history of economic thinking by economist-historians, that is, economic insiders.

In spite of the historiographic debates since the 1980s the greater part of what passes for 'history' of psychology does not live up to the demands of professional history (Coleman, Cola & Webster, 1993) and I contend that it had better be called 'history of psychological thinking'. Although the professional history of the field has become much more sophisticated over the years, as a theoretical psychologist I am more deeply concerned with the insider history of psychological thinking than with the outsider professional history and its particular strictures. Since it still holds true that many historical issues "are not distant problems for psychology" (Young, 1966, p. 21) I think it is important that such issues are studied by psychologist-scholars, reporting to psychologists. After all, in contradistinction to the former, professional historians work in a different field than psychology and "claim to have found a privileged view from the outside" (Kelley, 2002, p. 13). I have therefore never quite understood why the programmatic statements of critical historians, new historians, educated psychologists, or professional historians had to be so markedly exclusive. Would it not have been more productive to discuss what historiography is geared to what type of issue? Would not the 'new historians' have been well-advised to focus on the relative merits of the various historiographies rather than 'old history' bashing? And vice versa for that matter? Had these debates sounded a more inclusive tune, I cannot help thinking, the insider/outsider debates might have been more pragmatic-methodological and, perhaps, the fields of history and psychology might not have drifted apart (Van Rappard & Van Strien, 1993, p. 2).

In view of the above, it seems fitting to focus once more on Wundt. An additional reason is that, as we will be able to note in the following pages, Wundt features prominently in Danziger's oeuvre. In this essay, Wundt's mature psychology will be shown to allow description as a coherent system, based on activity-psychological assumptions. Moreover, this description is intended as an instance of the history of psychological thinking.

ACIVITY PSYCHOLOGY

Elsewhere I have argued, concurring with Danziger (1980a), that the German tradition in psychology whose philosophical foundations were laid in the writings of Leibniz and Hegel conceived of the mind as activity (Van Rappard, 1987; Bem & Van Rappard, forthcoming). This does not mean that the contents of the

mind, stressed by the empiricist 'way of ideas', were denied but that activity was considered the fundamental one of the activity-content distinction. This assumption did not function as a straight-jacket. Activity can be called a 'category', that is, paraphrasing Danziger (1997, p. 6), it operated at the level of the pre-understanding of that which a number of continental psychological theories were theories of. Operating at a deeper and more comprehensive level, the activity-category was spacious enough to accommodate a variety of psychological elaborations, which, however, tended to have a limited number of characteristics in common.

One of these characteristics comes to the fore in the general acknowledge-ment of the dynamic nature of consciousness. For instance, Wundt and Dilthey stressed consciousness as a formative activity. Activity as a formative force may be noted in some schools within the broad Gestalt-psychological movement too, even if the best-known one, the Berlin-school, happens to not quite fit in with the overall activity-scheme. Moreover, activity is seen in Brentano's 'acts', Külpe's 'determining tendency', Freud's 'libido' and, though less clearly, in Ebbinghaus too. To some extent a line may be drawn from the drive for self-unfolding in Leibniz and Hegel to activity in German psychology around 1900 as well as Vygotsky's Cultural Historical Activity Theory (Bem & Van Rappard, forthcoming). It is also worthy of note that activity provided what may perhaps be called the 'ground-layer of consciousness'. By this I mean that according to the activity tradition the mental contents that are introspected are never experienced as disconnected parts of the mind but as contents that, along with other contents, are situated in the dynamic ground of mental activity. Conceiving of activity as the ground-layer of the mental contents implies the existence of two layers or levels of consciousness: the level of the focally perceived contents on the one hand, and the ground-layer that one can only sense or be tacitly aware of on the other.

The lower level comprises a larger and more comprehensive field than the higher-level one, which tends to be identified with attention (apperception). At this level, the mental contents are attentively or focally known but also, and this is theoretically important, as 'originating from and hence still forming part of the ground-layer'. Even at the high level of apperception the mental contents are never experienced as isolated and therefore 'elemental' parts of consciousness but always as parts of a larger whole. The typically German recognition of the ground-layer of consciousness has brought with it the equally typical German stress on holism. As can already be seen in Leibniz, and on a grander scale in Hegel, consciousness is never a kaleidoscope of disconnected elemental contents but always an interconnected totality. Hegel stressed that since dialectical truth can only be found in the Whole (*das Ganze*), the interconnectedness of our knowl-edge was never to be lost sight of. Since the acquisition of empirical knowledge cannot but entail a curtailment and thus a distortion of the Whole, the object of study should be approached with great care. Because it should be left as much intact as possible, the researcher was urged to remain relatively passive. Instances

of such a 'methodological passivism' can be seen in Wundt, phenomenology, the human sciences, and Vygotsky and other Soviet psychologists. It comes to the fore in what nowadays is variously called, unobtrusive research, qualitative research, and 'object-adequate' research. The last term is especially telling because it draws the attention to the stress on the research object in contradistinction to the research method that can generally be seen in activity psychology. Faced with the perplexing difficulty of adequately studying human consciousness by means of natural-scientific methods, psychology has all too often one-sidedly stressed either the method or the object of investigation. Whereas activity psychologists tend to focus on the latter, their American colleagues put their money almost exclusively on method (Eisenga & Van Rappard, 1987). With regard to Wundtian psychology Danziger (1990, p. 36) noted a "tension between method and subject matter" but, as mentioned, such a tension is certainly not limited to Wundt. Moreover, speaking of a mere 'tension' would seem to be putting things mildly. Rather, as is particularly apparent in phenomenology and the human sciences, the words 'gap' or 'dilemma' would be more fitting since the investigators working along these lines were clear about their conviction that the natural-scientific approach by itself was incapable of adequately studying the mind. It was assigned a mere auxiliary role. Their solution to the 'object-method gap' typically was a firm stress on the object. A similar stress, although often in a somewhat weaker form, is characteristic of virtually all activity psychologists. It is of course part and parcel of their holism.

The characteristic topics of German psychology that were mentioned above may all be found in Wundt. But there is yet another one, typical of philosophers as Leibniz, Kant and Hegel, which has not yet been noted. I am referring to the conspicuous synthesizing tendency in their works. And surely, an attempt at the synthesis of various strands from the mental-activity as well as the mental-content traditions does also stand out in Wundt.

I am aware that pointing out the importance of the activity-category in Wundt cannot lay claim to being an original observation. In the late 1920s Boring (1957) has already mentioned much of what was said above, while Danziger (1980a, 2001a) too, is abundantly clear on it. The aim of this essay is to take activity-in-Wundt a step further by demonstrating that it lies at the basis of his psychology as a coherent system. That is, activity makes it possible to overcome what Danziger (1980d, p. 36) called, Wundt's 'conceptual duality'.

THE CONCEPTUAL FOUNDATION OF WUNDT'S PSYCHOLOGY

On the basis of his elaboration of Tetens's distinction between inner and outer perception (Van Rappard, 1976), Kant inferred that psychology was incapable of developing into a natural science. The attempts by early nineteenth century

psychologists as Herbart and Fortlage to escape from this verdict proved unsuccessful. The widely accepted inner-outer duality remained an obstacle. My description of Wundt's mature system will take its starting point in his concept of immediate experience, because it was geared to liberate psychology from its solitary confinement to the inner world which had by then lasted for almost a century.

Immediate Experience

As Wundt saw it, all sciences are grounded in experience—and this holds no less for natural science than for psychology. This well-known position entails that instead of basing the difference between natural science and psychology on their (outer or inner) domain, Wundt traced it to their perspective. In every instance of experience he assumed two aspects: the objects and the experiencing subject (*die Erfahrungsobjecte und das erfahrende Subject*). But these aspects are not disconnected. Wundt is not a dualist (Van Rappard, 1976). Subject and object merely constitute different perspectives on experience. The natural sciences study the objects of experience abstracting from the subject and, since their perspective is mediated by this abstraction they are called mediate. The significance of psychology may be seen in the fact that it nullifies this abstraction and thus studies experience in its non-mediated reality. Hence, psychology is called immediate and phenomenal (*anschaulich*). Often, the immediate-mediate pair is treated as essential for coming to grips with Wundt. I propose however, that it does not constitute the central theme, or rather, that the distinction is just one of several ways in which the central Wundtian theme comes to the fore. As will be elaborated below, Wundt's psychological system was built on a limited number of distinctions running parallel to the immediate-mediate pair. The most important ones are voluntarism-intellectualism, apperception-association, and psychic-physical causality. It will be demonstrated that these distinctions are all aimed at the synthesis of allegedly contradictory concepts by assuming them to be in what at this point can best be likened to a set-subset relation. I will return to this at a later stage. It bears repeating that the immediate-mediate distinction does not entail a duality. On the contrary, mediate experience may be thought of as that particular part of immediate experience that has been 'mediated' by scientific concepts. For instance, we may immediately experience that it is hot outside but if we wonder just how hot it is and look at a thermometer, the reading of 27 degrees centigrade is an instrumentally and conceptually mediated experience. The 'immediate' in Wundt's concept of immediate experience is equivalent to 'qualitative', while immediate experiences are similar to what is called in current debates in the cognitive sciences 'qualia' (e.g., Johnson, 1997).

In the description of immediate experience an important characteristic may be noted. Concurring with the Leibniz/Hegel view of consciousness as activity Wundt impressed upon his readers that "immediate, inner, or psychological experience" does not consist of static contents but is a dynamic interconnection of processes

(Wundt, 1918, p. 17). In *Psychic Causality and the Principle of Psychophysical Parallelism* (1894) he argued that the subject, "or to use its former term the soul (*Seele*)" is an ongoing interconnection of mental processes (p. 102). Moreover, and this adds another activity touch, this interconnection is also described as a synthesising process, a 'psychic synthesis'. Clearly, immediate experience, mental activity, interconnection, and the synthesising process are closely related.

Voluntarism

The fundamental activity-nature of the mind comes out particularly well in Wundt's voluntarism. Just like immediate experience, voluntarism brings out the dynamic nature and the interconnectedness of the mind. What the notion meant, was that it is primarily the volitional processes that are given in immediate experience and further, that this 'given' precludes the deduction of data from representations and other mental contents, as practised by intellectualistic psychologists (Wundt, 1893–5, III, p. 52). However, voluntarism was not intended to replace a bias towards cognition by another bias towards volition. What Wundt had in mind was rather to make room for the volitional processes and the feelings and affects connected to them. But since this had far-reaching consequences, 'making room' is putting it mildly. What Wundt intended was to establish a basis for a comprehensive view of mental life (*Gesamtauffassung des geistigen Lebens*), a basis which should form a supplement (*Ergänzung*) to intellectualism and which could be possible only on the basis of the recognition of feeling (*Gemüth*). He argued that psychology would remain incapable of getting an adequate picture of its aim until the discipline realised that volition constitutes the central process of the mind (Wundt, 1893–5, III, p. 165). Using the term 'voluntaristic psychology' Wundt suggested that the mind was to be understood in the light of the volitional processes, that is, as 'fleeting events' (*fliessende Ereignisse*).

Next to its dynamic, fleeting nature, the unity of the mind constitutes the second viewpoint advanced by voluntarism. In his *Definition of Psychology* (1896) Wundt maintained that the mental processes form a 'unitary process' (*ein einheitliches Geschehen*). Since representations, feelings, and will can only be approached as separate processes on the basis of analysis and abstraction, feeling and will, he said, were entitled to the same priority as sensation and representation, which had been stressed almost exclusively in the past (Wundt, 1896, p. 51). Moreover, since the various mental processes are inseparably connected parts of a single unitary process, the psychologist should keep in mind that it is in the synthesis of these parts that the basic condition of psychological research is found.

Activity and Process

The actuality and fleeting nature of the mind clearly point to Wundt's activity stance, and more indications will be seen below. But at this juncture I would like

to digress from the main line of my argument in order to demonstrate that activity psychology can be philosophically contextualised as 'process metaphysics'.

Just like the former, process metaphysics is a broad movement comprising many approaches. It can be traced to early Greek philosophy, especially Heraclitus, whose *panta rei* is a well-known, if not very substantial summary of process metaphysics. The guiding idea is that the furniture of the world can best be understood in terms of processes rather than fixed entities and that change is its predominant feature. As put by Rescher (1996, p. 8), "process is both pervasive in nature and fundamental for its understanding". The most important instance of process theory in early modern philosophy is Leibniz's *Monadology*. According to this metaphysics, the universe is composed of 'monads', which are 'centres of force', or bundles of activity. Monads possess an inner drive or *appetitio*, which makes for never-ending change. All monads develop according to a 'programmed harmony' as individual centres of activity operating at different levels of rationality within a cosmic whole. Hegel's dialecticism does not know stability either. Reality, now including human society, is seen as a process which continuously merges conflicting opposites into an unstable fusion. Dialecticism added the historical dimension that was still lacking in the *Monadology*. Other important processualists, as Rescher calls them, are William James and John Dewey. In 1903, the latter wrote to James, "It may be the continued working of the Hegelian bacillus of reconciliation in me, that makes me feel as if the conception of process gives a basis for uniting the truths [conceptual dualities] of pluralism and monism, and also of necessity and spontaneity" (in Rescher, 1996, p. 4). The philosopher Alfred Whitehead (1861–1947) is the inspiration of most current thinkers in process metaphysics. There are important links between his philosophy and Leibniz. Among other communal themes, Whitehead envisioned a 'philosophy of the organism' in that everything that exists not only forms part of an organic organisation but constitutes itself an organism too. "[It] is the pervasiveness of the growth/decay cycle operative throughout nature that marks this metaphysics of organism as being a metaphysic of process as well. The conception of an experientially integrated whole—a unit that is an organically systemic whole—represents a line of thought that links Whitehead closely to Leibniz and Bergson" (Rescher, 1996, p. 21).

As mentioned, process philosophy is a broad and hard to define movement. But a number of common characteristics can be stated, including interactive relatedness, wholeness, activity, and innovation/novelty (Rescher, 1996, p. 35). It is not difficult to relate these characteristics, at least in a general way, to crucial Wundtian notions such as, the interconnection of the mental processes and their nature of "*fliessende Ereignisse*", and concepts referring to creativity and novelty as creative synthesis, apperception, and psychic causality.

Why should it be important that Wundt's activity psychology allows philosophical contextualisation as process metaphysics? Although this needs more work and can only be lightly touched on in this essay, I think that approaching Wundt

as a process thinker might shed additional light on (1) the Wundt-James relation, and (2) immediate experience.

(1) James's well-known impatience with Wundt's work would seem to provide little reason to suspect that both may also have something in common. But this has been suggested by psychologists as wide apart as Judd (1905) and Hilgard (1987). The topic has been extensively treated by Danziger (1980d) as well. Indeed, it is not difficult to perceive similarities between Wundt and James, such as—limiting myself to some that were not mentioned by Danziger—the concepts of immediate and pure experience, their activity/process stances, and their roots in Romanticism (on Wundt, see Van Hoorn, forthcoming; on James, see Goodman, 1990). In view of James's irritation with Wundt, there is irony in Judd's (1905, p. 69) praise for the latter for having taken up "very much more fully [than James] the details of discussion which issue from his fundamental thesis". In sum, there seem to be more than superficial similarities between Wundt and James. And since the same holds for James and Whitehead (Eisendraht, 1971), it is not unlikely that similarities between Wundt and Whitehead may also be found.

(2) In his attempt to unsnarl the (mind/body) world-knot, the Whiteheadian philosopher David Griffin (1998) argued that process theory may offer an intelligible solution. Accordingly he constructed a view which claims to break down the dualism by demonstrating how mind and body share common characteristics. This is achieved by collapsing them into one category of momentary units of rudimentary sentience, which are initially subjective but may subsequently become objective. These units are construed as experiential, that is, as *events* rather than substances. As such, they provide the basis for understanding how mind and matter may arise from the same stuff. Clearly, this view entails a panpsychism, or, as Griffin prefers to call it, panexperientialism. Rather than, say, taking the insentient brain for a starting point and asking how conscious experience could possibly arise from it, the question should be, how does conscious experience arise from basic units that are themselves comprised of experience? It is important to see that consciousness is not considered a basic unit—it emerges in various degrees of complexity from a rudimentary sentience. Incidentally, perhaps a trace of the Leibnizian *petites perceptions* is perceptible here.

Whitehead defined nature strictly in terms of our direct intuitions. "What is not intuited but *only thought* is nature as consisting of absolutely insentient stuff or process. No such nature is *directly given* to us" (Griffin, 1998, p. 149, emphasis added). It seems to me that this process-perspective may help to understand the relation between Wundt's immediate and mediate experience.

Just as in process theory, in Wundt the mind is that which is directly or immediately intuited. But once that intuition is conceptually mediated, or 'only thought', it becomes a constant thing of the past. As Whitehead (1929/1978, p. 162) wrote, "those elements in our experience which stand out clearly and distinctly in our consciousness are not its basic facts; they are ... derivative modifications which [are] not the order of metaphysical priority". In other words, the mind/body problem can arise because we have taken an abstraction for a basic fact.

Now, in Wundt matter is not a substance existing next to and apart from mind. Whatever Wundt meant by 'psychophysical parallelism', it was not a dualism (Van Rappard, 1976). Fundamentally speaking, immediate experience is all there 'actually' is and it is from that 'stuff' that all conceptual things, both mental and material, are constructed. In other words, immediate mind is the present, mediate matter is a thing of the past. This enables me to elaborate on what was said earlier about the immediate/mediate relation being to some extent comparable to a set/subset relation: since mediate matter basically forms part of immediate mind, both belong to the same, single reality. Hence, the immediate/mediate relation may be likened to a set/subset relation because matter is a (mediate) construction out of the immediate mental. If there is any merit in this view, Wundt might well deserve a place among the 'still-active participants' in the mind/body debates currently raging in the philosophy of mind.

I now return to the main line of my argument.

Apperception

Earlier I observed that the central concepts of Wundt's psychological system are closely connected and it will shortly become clear that apperception is no exception to this. The apperception process occupies such a central place that Wundt opposed 'apperception psychology' to the intellectualistic psychology of Wolff, Herbart, and others that he rejected. Thus, from a foundational point of view apperception can be said to correspond to voluntarism. The apperception concept was gradually developed in the editions of the *Principles of Physiological Psychology* (Van Hoorn & Verhave, 1980). Wundt described apperception as "the psychological process that, when considering its objective aspect, consists of a clarification of the mental content and whose subjective aspect lies in certain feelings which, in relation to this content, are referred to as state of attention" (Wundt, 1911, I, p. 381, transl. vR).

With regard to the objective side of the apperception process, a distinction is found between the 'field of attention' (*Blickpunkt*) and the 'field of consciousness' (*Blickfeld*). The difference between the two concerns the degree of clarity and distinctness with which the mental contents are perceived. Apperceiving, some mental contents get more clearly outlined and hence, are distinguished as field of attention (also called, focus of consciousness, or inner focus) from the rest of the initial pre-apperceptive mental contents which may then be called, field of

consciousness (Wundt, 1918, p. 252). The attention is never directed towards a single content but always to a narrow field.

ASSOCIATION AND APPERCEPTION

The substance of the previous paragraph can be found in any decent text on the history of psychological thinking and there would be no reason to spend time on it, were it not for the association-apperception distinction. According to Wundt, the mental clarification process may assume two markedly different forms, association and apperception, whose point of difference provides an important building-block of his psychology as a coherent system.

This is not difficult to appreciate once it is seen that association and apperception are distinguished in the same way as the fields of consciousness and attention, respectively. The two pairs may be thought of as similar distinctions, which, however, stem from different perspectives or problem-contexts. Association, a concept referring to a particular form of thought-processes shares the feature of a lack of distinct delimitation with the field of consciousness, whereas such a delimitation is seen as distinctive of both the apperception and the attention-field.

The importance of delimitation can be demonstrated as follows: Apperception is subject to the 'binary law' (*Gesetz der Zweigliederung*), which Wundt also called the 'apperception law'. The law entails that each time the analysis of mental contents produces at least three parts, their connection is binary, that is, initially just two parts are distinguished of which one in turn acts as part to be bisected, and so on. In the categories of grammar the binary connection obtains as well. These categories always go back to two connected representations. A first analysis produces subject and predicate, after which the subject may be further analysed in, for instance, nomen and attribute (cf. Blumenthal, 2001). But in the associative train of thought representation B associates with A, after which C does the same with B, etc. Because it runs without direction, the A-B-C . . . association could in principle continue infinitely. It lacks delimitation. Apperception on the other hand, pursues a different course. At each stage of an apperceptive train of thought we are dealing with a distinctly delimited whole. The mental content in question is analysed into A, B, C . . . in such a way that at any point of the analysis only two parts are produced:
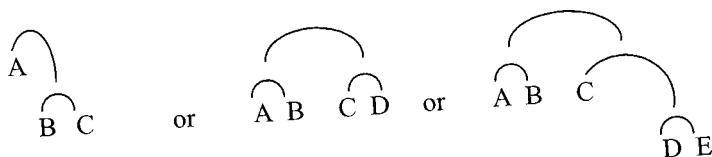
Clearly, in apperception no linear progression is found but rather a directed thought-process. In this direction the counterpart can be seen of the delimitation that marks the attention-field. Wundt emphasised that the apperception process is

always directed at the representation of an objective. One may of course abstract from the purpose of the apperception but only at the price of ending up with a heap of identical and hence elementary 'associative' connections—the initial meaningful whole of the apperceptive interconnections would be lost.

Whereas the parts of the association-chain are connected at random, the apperception-process tries to fit in with the previous connections and if possible even with the beginning of the train of thought. Apperception thus changes an associative chain into an interconnected structure of ideas (Wundt, 1893–5, I, p. 71). In some cases, the apperception may even add independently a (third) act of thought, for instance, A-C or C-A to the judgements A-B and B-C. It is not difficult to recognise here a syllogism.

Psychic and Physical Causality

We now come to the distinction between psychic and physical causality, which, together with the two key-distinctions discussed above forms the foundation of the conceptual structure erected by Wundt.

Contrasting psychic to physical causality, Wundt demonstrated the static nature of the connections abstracted from the stream of mental processes. Because of this abstraction, relatively permanent or constant (*beharrlich*) conditions are assumed as 'causes'. Such constancy also determines the form of physical causality, which concerns a relation between substantially thought and mutually reducible concepts. But reduction is not applicable to psychic causality. In this case, the connected conditions can be arbitrarily taken out of a complicated interconnection at any distance from each other without the need to consider the intermediate links.

Wundt's view on psychic causality may be understood on the basis of the holistic quality he thought characteristic of the apperceptive connections, as shown above, whereas physical causality was conceived as analogous with the associative connections. In contradistinction to the 'constant' physical causes abstracted from the stream of connecting mental processes, psychic causality is creatively intertwined with the whole of the *Seele*. And yet another similarity between psychic causality and the apperceptive connections may be pointed out: both aim at creating connections that are as complete as possible. Wundt thought that psychic causality was typical in that near and distant mental contents (*Nahes und Entferntes*) are interconnected in the same manner and this, of course, entails that there can be no linear cause-and-effect chain. On the contrary, it is possible that causal conditions that are far off in time exert a larger influence than more recent causes. In a psychic-causal interconnection, the scientific abstractions of temporal contiguity and *causa aequat effectum* make way for the immediately given reality of the (apperceptive) intertwining of all connections. What obtains in such cases is, put in the terms of Dilthey, a dynamic *Lebenszusammenhang*. In his *Introduction to Psychology*, Wundt therefore called the psychologist "a prophet turned towards

the past". From the very beginning (Wundt, 1862) one of the focus points of his interests was the development of this interconnection (*Zusammenhang*), which he also called 'creative (apperceptive) synthesis'. With regard to *Völkerpsychologie*, it is important to note that this interconnecting process is not, according to Wundt, limited to the individual mind but also 'creatively synthesizes' the super-individual minds or volitional communities.

Summarising this section on the conceptual foundations of Wundt's psychology, I contend that the various key-distinctions that were introduced all appear to be most intimately interconnected and, more importantly, they all run parallel to each other in such a way that they can be said to come down to a single crucial idea, approached from different angles. One way to phrase this idea might be that all distinctions are geared to safeguard in their own province that the psychological and the natural-scientific perspectives are mutually irreducible. Thus, the independence of psychology would seem guaranteed.

PSYCHOLOGY

I will now continue the description of Wundt's psychology as a coherent system taking the perspective that it comprises two markedly different approaches, which, however, correspond with the distinctions discussed above. These psychologies are of course, the *Psychophysical Experiment* and the *Völkerpsychologie*, which will be shown to run parallel with mediate and immediate experience (and related distinctions), respectively.

I mentioned earlier that Wundt did not accept Kant's distinction between inner and outer perception but instead took immediate experience for his starting-point. The immediate/mediate and the inner/outer distinctions are tangential since the former was intended to bridge the gap entailed by the latter. However, Wundt did agree with Kant and many others, that unqualified introspection was incapable of yielding data of scientific significance. He therefore distinguished between the traditional type of introspection as found in eighteenth and nineteenth century psychology on the one hand, and 'inner perception' on the other. Given his rejection of the duality entailed by the concept of inner/outer perception, Wundt's use of the term inner perception would seem somewhat unfortunate.

As the story is told by Danziger (1980b, 1980c, 1990), in order to be able to establish a scientific, that is, experimental psychology, the transformation was required of inner perception into something like scientific observation. Wundt therefore conceived of a way to manipulate the conditions of inner perception so that they approximated the conditions of 'external' observation. This approximation is essentially what constituted the Psychophysical Experiment. Another important requirement for a scientific approach to inner perception is that the specific experiences obtained by a particular research project allow replication in order to

observe them. The laboratory offers the possibility to produce sufficiently similar subjective experiences by means of simple, identical external stimuli. "The general idea was that inner perception could yield acceptable data for science only insofar as experimental conditions permitted a replication of inner experience at will" (Danziger, 1990, p. 35). But this could only be achieved at the cost of restricting the scope of the experimental approach since the experimental conditions were necessarily limited to simple sensory stimuli. Only physical stimuli lent themselves to uncomplicated identical repetition and hence could be assumed to trigger identical experiences.

Clearly, the Wundtian experiment was a highly controlled affair. Only well-trained observers were allowed into the laboratory, where they were presented with carefully controlled physical stimuli. Moreover, they were required to report their inner perceptions in terms of a very restricted vocabulary, consisting largely of size, intensity, and duration. Recalling the example given above of the observation 'it is hot outside' as an instance of immediate experience and 'the temperature is 27 degrees' as an instance of mediate experience, it will be clear that the experimental subjects had to report in terms of mediate experience. Thus, not only the psychological domain covered by Wundt's experiment is restricted but its experiential range is severely limited too. But all strictures were geared to making the observer 'master' of the seemingly unmanageable mental flux.

> "(The psychophysical experiment) creates external conditions that look towards the production of a determinate mental process at a given moment [and] it makes the observer so far master of the general situation, that the state of consciousness accompanying this process remains approximately unchanged." (Wundt, 1910, p. 45)

Although it is possible that we basically agree on this point, with regard to this quotation I would not, as Danziger did above, explain the function of the experimental conditions as an attempt to make inner perception approximate external observation. Rather, my activity point of view makes me think that Wundt's experimental procedure is intended to make the otherwise unmanageable stream of consciousness fit for observation by bringing it to a stop. The *fliessende Ereignisse* of the mind allow observation only to the extent that they are, as Wundt said above, "approximately unchanged". However, unchanged (*beharrlich*) is precisely what the mind, according to his foundational assumptions, is not. Thus, the psychophysical experiment is a paradoxical affair: On the one hand, it claimed to make possible the experimental, that is, scientific study of the traditionally enigmatic topic of consciousness. On the other hand, however, the design of the experiment required the subjects to report their inner perceptions in terms of "unchanged" or mediate experience, whereas consciousness had been defined in terms of immediate experience. Phrased yet differently, consciousness, which according to Wundt constituted a stream, activity, or process was, for reasons of methodological feasibility investigated in a way that was incapable of coming to grips with its essential features. For methodological reasons activity/process had been reduced to static,

'experimentally frozen' contents. That is, the integrity of the object had succumbed to the demands of method—a clear instance of the object-method gap mentioned above. In *Völkerpsychologie*, however, consciousness-as-activity could, according to Wundt, be studied in a way that was rather more adequate to its object.

Stressing the limited scope of the Wundtian experiment provides a useful starting point for going into the division of labour between the experimental and *Völker*-psychological approaches.

When a sensory stimulus enters the field of consciousness it may be perceived or 'apprehended'—if the attention then focuses on it, the perception of the stimulus is brought to the attention field where it is apperceived. The apprehension is passively determined by the stimulation, along with the prevailing physiological and psychological conditions of the subject. One responds to a stimulus quite automatically and it is 'mindlessly' associated with the other mental contents which happen to be around in the consciousness-field at that time. Thus, the experimental manipulation of experience is geared to the relatively low level of mental activity, which is characteristic of the field of consciousness. Only processes of short duration that can be triggered by simple sensory stimuli, that is, sensation and perception, met the conditions for experimental scrutiny.

In contradistinction to apprehension, apperception is assumed to be a voluntary act. Once the attention is focused on a stimulus, it is 'thoughtfully' apperceived and thus gets woven into a mental interconnection or structure. It is at this point that thinking takes place, as distinguished from mere association. Although attention proved to some extent amenable to experimental manipulation, thinking was not. Apperceptive thought, in which the creative synthesis came to the fore could not be linked to the simple stimuli used in psychophysical experimentation. Thus, a general partition is seen to emerge between the field of consciousness, research topics as sensation, perception, and simple affective processes, and the psychophysical experiment on the one hand, and the field of attention, research topics as thinking, and the more complex affects, and the 'historical method' of *Völkerpsychologie* on the other hand.

VÖLKERPSYCHOLOGIE

Wundt's earliest notions of what was to become the *Völkerpsychologie* some forty years later (Wundt, 1863) built on the work of Lazarus and Steinthal in the 1850s. The premises of their work have been summarised by Eckardt (1988) as follows:

1) Humans are societal (*gesellschaftlich*) beings and thus are determined by the 'social whole' (*Gesamtheit*).
2) The determining factor of the societal nature of individuals is the collective mind of the people (*Volksgeist*).
3) The essential form of human society is the people (*Volk*).

As will be clear from this summary, the work of Lazarus and Steinthal can hardly be called 'psychology'. Even by the standards of the mid-1800s it lacked a psychological perspective.

Starting in his late sixties, Wundt finally published ten volumes of *Völkerpsychologie*, variously translated as Folk-, Group-, Social-, Cultural-, or Ethnic Psychology. For all their inadequacy, these translations indicate that *Völkerpsychologie* was designed to study super-individual functions. In language, being a product of the super-individual or collective mind the operation of these functions may be observed. The study of language and language-as-objectified in custom, myth, religion, and other products of the collective mind on the one hand, and psychophysical experimentation on the other hand, were not conceived as disconnected approaches. It is worthy of note that the latter was not designed to test hypotheses. It was what is currently called, a demonstration experiment, that is, it was designed to 'demonstrate' certain mental phenomena and make possible their observation by stopping the mental flux. But in *Völkerpsychologie* the constancy required for observation cannot be created experimentally because of the limited scope of the experimental stimuli. Fortunately however, it is possible to observe the collective mental functions indirectly in the historical development of their linguistic products, and this is precisely what the 'historical method' amounts to.

Danziger (1983, p. 306) mentions that early in his career Wundt did already entertain the notion of the necessary complementarity of experimental and *Völkerpsychologie*. In a later paper (Danziger, 2001a, pp. 85 ff.), this complementarity is explained by Wundt's conviction that basically there are only minds in interaction. I have no reason to dispute this explanation but I would like to propose, in keeping with the aim of this essay, that another and perhaps stronger connection between experimental and *Völkerpsychologie* may also be inferred: Both are geared to the same goal of making possible the 'observation' of the mind, even if they do this in different but complementary ways. As Danziger points out, Wundt never abandoned his conviction that experimental psychology "needed to be supplemented by a branch of psychological studies that was devoted to the study of human mental processes in their social aspects; and . . . that this latter type of study was able to make use of data that were no less objective than the data of experimental psychology" (Danziger, 1983, p. 307).

Wundt has tried to transform the work of Lazarus and Steinthal along more psychological lines. Nevertheless, according to Danziger (1983) and Eckardt (1988) the result did not get beyond the programmatic stage and never developed into an empirical psychology worth the name. An obstacle that Wundt was unable to overcome is the theoretical distance between super-individual history on the one hand, and intra-individual psychology on the other. It is difficult to draw reliable conclusions about the psychological processes that determine human interactions on the basis of historical analyses of the objectivations of these interactions. Apart from this foundational problem it must also be taken into account that actually,

Wundtian *Völkerpsychologie* did not investigate human interactions because language was only studied as expression (*Ausdruck*). As argued by Nerlich and Clarke (1998), the *Völkerpsychologie* was no more than an *Ausdruck* psychology and social interaction had no place in it. Danziger has reached a similar conclusion. "The psychologically relevant environment", he notes (Danziger, 2001a, p. 89), "was *geistig*, i.e. mental or spiritual". This was of course not the direction in which twentieth century social psychology was to develop. Nevertheless, in spite of "its gross limitations it did contain some hints of alternative directions of conceptual development which social psychology may have ignored to its cost" (Danziger, 1983, p. 311).

In the context of this paper, this statement may serve as another example of Wundt as a 'still-active participant'.

CONCLUSION

In this essay I have attempted, firstly, to describe Wundt's psychology as a coherent system and, secondly, to use this system as a point in case for the history of psychological thinking.

With regard to the first goal, I think I have demonstrated that the Wundtian key-concepts of immediate experience, voluntarism, apperception, psychic causality, and creative synthesis are interconnected, or rather, that they were all intended to do the same job in their respective domains, namely, to safeguard the primacy of mind. Hence, I agree with Danziger (1980d) that Wundt was really more of a philosopher than a psychologist. Perhaps it is better to avoid the noun 'mind' when speaking about his philosophy and use 'mental activity' instead. After all, in Wundt's view, mind is emphatically not a constant entity but activity or process. Now you may wonder, which one of the key-concepts mentioned should be considered foundational for the coherence of his psychology? But I think that this question defies answering. When I was writing a thesis on eighteenth and nineteenth century German psycho-philosophy (Van Rappard, 1976), I noticed to my despair that the key-concepts dug up from the gothic print were so closely interconnected that it seemed that between them they formed a tight circle. You may go around it many times, wondering at what point you might break it. But eventually you come to realise that it does not really matter which link you try because by virtue of their intimate connection any one may provide an entrance to the whole. Creative synthesis may have been an important guide for Wundt when developing his psychological ideas (Blumenthal, 2001, p. 130), but it is not to be taken as the one foundational concept of the mature system. As I see it, it is immaterial which key-concept you take as the foundational one—any one will do. But underlying all of them, I contend, is the intuition of mind as activity or process. Phrased differently, Wundt's key-concepts may all be considered specific

forms, geared to their respective domains, in which mind-as-activity/process may be seen to shine through.

The second goal of this essay concerns the relevance of history for the psychological discipline. As mentioned in the Introduction, this essay is intended as an instance of the history of psychological thinking as distinguished from the history of psychology. I maintain that scattered through Danziger's writings many observations can be found that can be assigned to the former category. To give just one example,

> "The history of psychology does not involve the progressive development of a single discipline but rather the often simultaneous appearance of a number of different disciplines, each one of which defined its object of study in a different way. Such definitions predetermine the range of findings and interpretations that is possible for a discipline. A historical examination of alternative foundations therefore provides a way of transcending the narrow horizons that confine the more dogmatic adherents of any particular disciplinary matrix." (Danziger, 1983, p. 303; other examples can be found in, e.g., Danziger, 1979, p. 205; 1980a, p. 86; 1980d, p. 378; 1985, p. 133; 1994, pp. 471–472; 2001a, 2001b, 2001c, p. 46)

As I see it, in this quotation as well as in the other examples the historian of psychology is pictured as a participant in theoretical-psychological debates. In this essay, two conceivable opportunities for Wundt to take part in such debates have been pointed out, while Danziger (2001a, p. 73) mentions several other general items. Now, it is in this use of our classics, I contend, that the relevance of history for psychology may be seen in particular. I think that Danziger would agree even if he has reservations, wavering between the stances of the scientist-scholar and the professional historian. "Historical studies pursued by active practitioners of a discipline often suffer from a tendency to look for precursors of present day viewpoints or anticipations of current theoretical positions", he wrote (Danziger, 2001a, p. 92). He then reassured us that this is "quite understandable if one's primary engagement is with to-day's issues", adding however, "but it does not make for very good history".

I agree—and in the preceding pages I have tried, among other things, to explain why.

REFERENCES

Barbalet, J.M. (1999). William James' theory of emotions—Filling in the picture. *Journal of the Theory of Social Behaviour, 29,* 251–266.

Bem, S. & Rappard, J.F.H. van (forthcoming). *Going for consciousness* (provisional title). London: Sage.

Blumenthal, A.L. (2001). A Wundt primer—The operating characteristics of consciousness. In R.W. Rieber & D.K. Robinson (Eds.), *Wilhelm Wundt in history—The making of a scientific psychology* (pp. 121–144). New York: Kluwer Academic/Plenum Press.

Boring, E.G. (1957). *A history of experimental psychology*. New York: Appleton-Century-Crofts.

Coleman, S.R., Cola, Ph. & Webster, S. (1993). Empirical assessment of methodology in the history-of-psychology literature, 1975–1986. *American Journal of Psychology, 106*, 253–257.

Danziger, K. (1979). The positivist repudiation of Wundt. *Journal of the History of the Behavioral Sciences, 15*, 205–230.

Danziger, K. (1980a). Wundt and the two traditions in psychology. In R.W. Rieber (Ed.), *Wilhelm Wundt and the making of a scientific psychology* (pp. 73–87). New York: Plenum Press.

Danziger, K. (1980b). Wundt's theory of behavior and volition. In R.W. Rieber (Ed.), *Wilhelm Wundt and the making of a scientific psychology* (pp. 89–115). New York: Plenum Press.

Danziger, K. (1980c). The history of introspection reconsidered. *Journal of the History of the Behavioral Sciences, 18*, 241–262.

Danziger, K. (1980d). On the threshold of the new psychology—Situating Wundt and James. In W.G. Bringmann & R.D. Tweney (Eds.), *Wundt studies* (pp. 363–379). Toronto: Hogrefe.

Danziger, K. (1983). Origins and basic principles of Wundt's *Völkerpsychologie. British Journal of Social Psychology, 22,* 303–313.

Danziger, K. (1985). The origins of the psychological experiment as a social institution. *American Psychologist, 40*, 133–140.

Danziger, K. (1990). *Constructing the subject—Historical origins of psychological research.* Cambridge: Cambridge University Press.

Danziger, K. (1994). Does the history of psychology have a future? *Theory & Psychology, 4*, 467–484.

Danziger, K. (1997). *Naming the mind—How psychology found its language.* London: Sage.

Danziger, K. (2001a). Wundt and the temptations of psychology. In R.W. Rieber & D.K. Robinson (Eds.). *Wilhelm Wundt in history—The making of a scientific psychology* (pp. 69–94). New York: Kluwer Academic/Plenum Publishers.

Danziger, K. (2001b). The unknown Wundt—Drive, apperception, and volition. In R.W. Rieber & D.K. Robinson (Eds.), *Wilhelm Wundt in history—The making of a scientific psychology* (pp. 95–120). New York: Kluwer Academic/Plenum Publishers.

Danziger, K. (2001c). Sealing off the discipline—Wilhelm Wundt and the psychology of memory. In C.D. Green, M. Shore, & T. Teo (Eds.), *Psychological thought in the nineteenth century—The transition from philosophy to science and the challenges of uncertainty* (pp. 45–62). Washington, D.C.: American Psychological Association.

Eckart, G. (1988). Die frühe Völkerpsychologie—Wegbereiter oder Hemmnis für die Enstehung einer wissenschaftlichen Sozial- und Entwicklungspsychologie? In S. Bem & J.F.H. van Rappard (Eds.), *Studies in the history of psychology and the social sciences, 5* (pp. 192–201). Leiden: DSWO Press.

Eisendraht, C.R. (1971). *The unifying moment—The psychological philosophy of William James and Alfred North Whitehead*. Cambridge (MA): Harvard University Press.

Eisenga, L.K.A. & Van Rappard, J.F.H. (1987). *Hoofdstromen en mensbeelden in de psychologie*. Meppel/Amsterdam: Boom.

Goodman, R.B. (1990). *American philosophy and the romantic tradition. Cambridge: Cambridge University Press.*

Greenwood, J. (1999). From Völkerpsychologie to cultural psychology. *Philosophical Psychology, 12*, 503–514.

Griffin, D.R. (1998). *Unsnarling the world-knot—Consciousness, freedom, and the mind-body problem*. Berkeley: University California Press.

Haldane, J. (2000). Thomas Reid and the history of ideas. *American Catholic Philosophical Quarterly*, *LXXIV*, 447–469.

Harré, R. (2000). Personalism in the context of a social constructionist psychology—Stern and Vygotsky. *Theory & Psychology, 10*, 731–748.

Hilgard, E.R. (1987). *Psychology in America—A historical survey*. San Diego: Harcourt, Brace, Jovanovich.

Hoorn, W. van (2002). Goethes Gleichnisrede der psychischen Chemie als romantischer Hintergrund zu Wundts experimenteller Psychologie. *Psychologie und Geschichte, 10*, 233–246.

Hoorn, van W. & Verhave, Th. (1980). Wundt's changing conceptions of a general and theoretical psychology. In W.G. Bringmann & R.D. Tweney (Eds.). *Wundt studies* (pp. 71–113). Toronto: Hogrefe.

Johnson, B. (1997). Dennett on qualia and consciousness—A critique. *Canadian Journal of Philosophy, 27*, 47–82.

Judd, Ch. J. (1905). Radical empiricism and Wundt's philosophy. *Journal of Philosophy, Psychology and Scientific Methods, 2*, 169–176.

Kelley, D.R. (2002). Intellectual history and cultural history—The inside and the outside. *History of the Human Sciences, 15*, 1–19.

Kuklick, H. (1999). Assessing research in the history of sociology and anthropology. *Journal of the History of the Behavioral Sciences, 35*, 227–237.

Nerlich, B. & Clarke, D.D. (1998). The linguistic repudiation of Wundt. *History of Psychology, 3*, 179–204.

Rappard, J.F.H. van (1976). *Psychologie als zelfkennis*. Amsterdam: Academische Pers. (Also published as Psychology as self-knowledge. Transl. L. Faili. Assen (Neth.): Van Gorcum, 1979).

Rappard, J.F.H van (1987). Handelingspsychologie—Ontwerp voor een historisch ordeningskader. *Handelingen, 1*, 14–29.

Rappard, J.F.H. van (1997). History of psychology turned inside(r) out. *Theory & Psychology, 7*, 101–105.

Rappard, J.F.H. van & Strien, P.J. van (1993). History and theory—Introduction. In: J.F.H. van Rappard, P.J. van Strien, L.P. Mos & W.J. Baker (Eds.). *Annals of Theoretical Psychology, Vol. 8* (pp. 1–14). New York/London: Plenum Press.

Rescher, N. (1996). *Process metaphysics*. Albany: SUNY Press.

Whitehead, A.N. (1929/1978). *Process and reality—An essay in cosmology*. Corr. Ed. D.R. Griffin & D.W. Sherburne. New York: Free Press.

Wundt, W. (1911). *Grundzüge der physiologischen Psychologie* (5th edition), 1. Band. Leipzig: Engelmann Verlag.

Wundt, W. (1862). *Beiträge zur Theorie der Sinneswahrnehmung*. Leipzig: Wintersche Verlag.

Wundt, W. (1863). *Vorlesungen über die Menschen- und Thierseele*. Leipzig: Voss.

Wundt, W. (1893–1895). *Logik, eine Untersuchung der Prinzipien der Erkenntnis und der Methode wissen schaftlicher Forschung, 3 Bde*. Stuttgart: Ferdinand Enke.

Wundt, W. (1894). Über psychische Kausalität und das Princip des psychophysischen Parallelismus. *Philosophische Studien, X*, 1–124.

Wundt, W. (1896). Über die Definition der Psychologie. *Philosophische Studien, XII*, 1–66.

Wundt, W. (1900). *Völkerpsychologie, eine Untersuchung der Entwicklungsgesetze von Spache, Mythus und Sitte, 1. Bd. Die Sprache*. Leipzig: Engelmann.

Wundt, W. (1910). *Principles of physiological psychology* (Transl. E.B. Titchener, Part 1 of 5th Edition), New York: Macmillan.

Wundt, W. (1912). *Elemente der Völkerpsychologie. Grundlinien einer psychologischen Entwicklungsgeschichte der Menschheit*. Leipzig: Kröner.

Wundt, W. (1918). *Grundriss der Psychologie*. Leipzig: Engelmann.

Young, R.M. (1966). Scholarship in the behavioural sciences. *History of Science, 5*, 1–51.

Zuriff, G.E. (1985). *Behaviorism—A conceptual reconstruction*. New York: Columbia University Press.

THE MISSING LINK OF HISTORICAL PSYCHOLOGY

Willem van Hoorn

"Of all the approaches towards man, psychology is the most unlikely"
Roland Barthes

INTRODUCTION

A critical historiography of *scientific* psychology has to deal with the social and *cultural* movements in which the discipline developed as a practical social technology, an intellectual enterprise and as a profession on a par with the medical and engineering professions.

In this essay a plea is made for considering historical psychology as an integral part of the historiography of scientific psychology. The very historicity of emotions, feelings, experiences, cognitions, behaviors and interpersonal relations, has its consequences for writing history of psychology as such. After all we are only talking about *one* century of psychology: 1879–1979. However, what do we mean by 'historicity' when it comes to assessing the proper subject matter of psychology? And, what do we mean by 'historicizing the subject'? That women and men are social and cultural-historical beings through and through is already recognized for some time. Now we have to ascertain in what this historicity precisely consists. One preliminary answer to that basic question could be that a particular kind of *romantic subjectivity*, which emerged around the turn of the nineteenth century, formed a cultural prerequisite for the founding of practical and theoretical psychology in the 'laboratories' of Galton and Wundt.

Starting from the distinction between the historiography of scientific psychology and some principles of historical psychology, I will argue that both disciplines have one common root, *viz.* the *life-world* (Husserl; Merleau-Ponty). The life-world is conceived of as the daily world of our lived-through and shared experiences that make up common coordinated actions in which life itself unfolds, develops and dies away in the course of social time. With regard to the life-world we can note that the experiencing self constitutes its nucleus and that the individual's biographical situation forms its cultural historicity. To vary a celebrated quote from John Locke: there is nothing in scientific psychology that was not first in the life-world. But, how does the life-world get into psychology?

Within the self the world is mentally/bodily structured through peculiar patterns of 'near', 'safe', 'familiar smell', 'home ground' and 'far away', 'strange' or 'foreigner's ground'. The self's frames of reference also contain: worries about health and well-being, confidence, 'otherness', mistrust, a sense of one's own body and time perspectives like social past and future that are linked to collective and *autobiographic* memories (Galton, 1879). One of the basic characteristics of the contemporary self is an unprecedented preoccupation with (*secular*) *inner life*. The latter is no doubt a romantic innovation, like so many other legacies of Romanticism that partly transformed into *psychological objects* during the 20th century. Most importantly among these were *Empfindungen* (sensations) and *Gefühle* (feelings), which eventually became objects of psychological inquiry (Danziger, 1997, p. 49). 'Simple feelings' and 'pure sensations' constitute the psychical elements of Wundt's experimental psychology, as will be worked out in greater detail later.

Hence, whatever else the discipline may have been named during that one century of psychology, to me it remains—paradoxically—the science of the individual. However, the individual is not an atom amongst social atoms in an anonymous society, but a person among other human beings to whom one is *interpersonally* related. By linking the social and cultural time-dimensions to the interpersonally determined biographical situation, we enter into *generational transformation*. Nobody in his sound wit will purport to fully understand his own generation. Our parents and grandparents lived different and often difficult lives that we might partly try to grasp. "My parents did not yet know, as nobody knows, at which position they were located in history. And one thing they did not know at all: that their life enrolled between a bygone World War and an emerging one" (Mak, 1999, p. 191; translation WvH). People who lived more than two centuries ago may almost look like strangers to us. Historians who think that they perfectly well understand the lives of medieval people are in need of historiographic therapy (Le Goff, 1977/2001). These reflections bring us to the heart of historical psychology, namely, the elucidation of diachronous discontinuously changing life-worlds in which the vulnerable *sensitivities of the human subject* always play their key role. Without exaggeration we could call the unraveling of the ever-changing Western

subjectivities during this century of psychology, one of the main tasks of the professional historians of the discipline. Psychological *knowledge* is always conceptualized in a specific social-psychological setting, as Danziger has amply made clear. Contemporary personal subjectivity as it took its first unstable shapes during the Romantic period, undoubtedly rises out of new interpersonal relationships. Here we may think primarily of inter-sexual relations, of relationships of capital provider and laborer, of parents and children, of siblings and also of schoolmates. Those new interpersonal relationships are firmly grounded in *material* culture: technics and technology, the built environment, artifacts and artifices, the basics of economics and law and last, not least, the achievements of practical medicine that help cure the sick and prolong life itself on a hitherto unexpected scale.

In this essay historiographical tools such as the notions of *proto-psychology*, of mindscape, synchronism and anachronism in the life-world, the discontinuity of inter-subjectivity and the invisibility of present day psychology, will be successively dealt with.

Barthes' challenging epigram may stimulate us to now find out why the most unlikely approach towards the understanding of people has become one of the commonest ways of dealing with human comings and goings during three quarters of the 20th century.

THE INHERENT RELATIONSHIP OF SCIENTIFIC PSYCHOLOGY AND HISTORICAL PSYCHOLOGY

"...That up till now we have had no historical psychology...Why did the Middle Ages and the Renaissance produce entirely different *types* of men?"

Karl Mannheim

More than forty years ago I began to study psychology under Jan Hendrik van den Berg, now 90, the Dutch pioneer in historical psychology, who published his groundbreaking book *Metabletica* as early as 1956. Metabletica, or historical psychology, starts from the basic idea that there is no fixed 'human nature'. Or, as Van den Berg has put it, to reject: "...the characteristic of the *eternity value* of 19th C. psychology (...). The whole science of psychology is based on the assumption that man does not change (...) whereas, in traditional psychology, the life of a previous generation is seen as a variation on a known theme, the supposition that man does change leads to the thought that earlier generations lived a different life, and that they were *essentially different*" (1956/1961, pp. 7–8 italics added). We may assume that his contention of the *openness* of human existence has been borrowed from Martin Heidegger and Maurice Merleau-Ponty. In the latter's 1945 brilliant study of human existence we read: "Man is an historical idea and not a natural species" (p. 199, translation WvH). In Merleau's vision *all* psychological knowledge is based on the body's primordial encounters with other people. The

value of life "consists in actively being what we are by accident, to establish a communication with others and ourselves for which our *temporal structure* gives us the chance and of which our freedom is only a sketch" (1966, p. 71 italics added; translation WvH). Thus, it is the ever-changing materiality of the body, its socio-cultural conceptualizations and the lived-through experiences of one's own body and the bodies of others, which principally defy the construction of long-standing psychological theories. Since there is an inextricable bond between the body and our lived-through experiences, each theory in psychology should be provided with an indication of its expiry date.

In my view, 20th century scientific psychology can only be conceived of as the last stage of historical psychology. As is almost too well known, it was only by the middle of the 19th century that, fully unexpectedly, scientific psychology emerged as the 'discipline' that studied the *psychical* contents of human *consciousness: Bewusstseinspsychologie*. With the explicitly proclaimed 'death of God' by thousands of cultural revolutionaries, the Christian soul had seemingly become obsolete. In its place subjective consciousness, feelings, emotions and the individual psyche became ready to play their grand roles in the theater of body and mind. Immediately, 'the unconscious', joined them and this then made the new subject matter of psychology complete: *Tiefenpsychologie.* In this connection we would need to thoroughly deal with the legacy of Arthur Schopenhauer (1788–1860) who in his sublime work of 1819 *The World as Will and Representation* clearly summarizes the *transformations* of 'soul' into 'psyche' and of 'sex' into 'sexuality', which took place under his very eyes. Writes Schopenhauer: "The sex drive is the final goal of all human strivings. With its accomplishment everything is accomplished . . . (. . .) As physiological correlate of the *subjective* sex drive we find in the human organism sperm as the secretion of all secretions, the quintessence of all saps. This fact shows us again that the human body is only objectified will, i.e. the will itself viewed through the intellect" (1819, p. 603, my transl.).

We should take breath for a few moments and pause to admire the psychologically interesting aspects of Schopenhauer's position. First, in the truest of all romantic veins, the philosopher concentrates upon the *experiential* aspects of the sex drive: transformation of 'soul' into 'psyche'. Feelings, emotions and sensuousness are directly related to intercourse and thus, here, sex partly changes into sexuality by giving value to both its psychical and physiological characteristics. The subjective and the objective parts of the drives of all drives, a romantic notion by itself, are theoretically set apart and then concretely united again in the love act of two people.

'*Behavior*', conceived of as a collection of bits and pieces of associations of stimulus-response relationships, is a genuine 20th century invention (Danziger, 1997, pp. 85–110). Like parts of 'sex' transformed into sexuality and parts of the 'soul' transformed into psyche, parts of 'conduct' transformed into objectified 'behavior'. However, we cannot say in honesty that the results of thousands of behaviorist studies have greatly enriched our understanding of what truly drives

women, men and children. For these *matters of the heart* (Alexis de Tocqueville) we have to consult the works of some honest psychoanalytic writers and poets, playwrights and novelists. It is remarkable that Danziger in his recent work on the 'origins' of psychological categories (1997) has not paid much attention to the positive results of the psychoanalytic movement. In comparison to the achievements of 'scientific' psychology there can be little doubt that Freud's legacy has had rather far-reaching 'influences' on cultural, artistic and personal Western life in the 20th century Or, to put it more interestingly, has secularized, sexualized, individualized and personalized bourgeois life delivered the material for the production of psychoanalytic knowledge? Who knows what an as yet to be written *archeology* of psychoanalytic categories would reveal in this respect? What surprises thus a little is that Danziger has not given a glimpse of the core-concept of *repression.* There can be no serious doubt that Freudian psychoanalysis and its present day therapeutic practices stand or fall with the validity or invalidity of that often-misused notion. Likewise, Danziger has not, as yet, valued the basic tenets of for example phenomenological psychology and Gestalt psychology. Decades before American social constructionism became en vogue, phenomenological psychology has laid down its anthropological foundations of which the very historicity of human existence itself forms the cornerstone (Giorgi, 1970; Delphy & Chaperon, 2002). As Danziger himself admits, one cannot master the whole field of theoretical psychology (1997, p. 20). However, his 'neglects' clearly indicate that he is primarily dealing with what the majority of psychologists would call 'scientific' or simply *American* psychology.

If we apply the foregoing to the historiography of psychology as a whole, then we may distinguish between *universals* and *particulars* with regard to outlining psychology's course and contents from 1800 till 1979:

Universals of social, cultural and economic development:

— The ongoing industrial and post-industrial revolutions
— The autonomization of technology
— Rise and continuing influence of the cultural movements of Romanticism, Realism and Symbolism
— Wars and World Wars as perennial strife
— The rise to world power nr. 1 of the United States from 1900 on
— The domination of the West: colonization/de-colonization and its aftermath
— Secularization/urbanization and the counter-forces of religious fundamentalism and regionalism
— The prolongation of life through preventive and applied medicine

Particulars of Western psychological development:

— The emergence of the romantic inner self: transformation of 'soul' into psyche

— The transformation of 'sex' into sexuality
— The increasing importance of sexuality and male and female homosexuality as gender issues
— Emergence and significance of unconscious psychical processes
— The emergence of 'behavior' as an objectified part of human conduct
— The split of theoretical psychology and practical psychology
— The almost world-wide dominance of *American* psychology after World War II

One factor should be particularly highlighted: the undetermined status of subjectivity itself. When the age-old passions partly faded away by the end of the 18th century, feelings and emotions gained prominence and modern psychological man and woman were born. The passions had led to the medieval recognition of *types* of man (sanguine, phlegmatic, choleric and melancholic). Universal passions were partly replaced by singular feelings and emotions (Paris, 2001). The existence and experienced reality of 'infinite' emotions and feelings lead to the recognition of the uniqueness of each individual.

In sum then, through combining the historical psychology approach with some basic principles of the cultural movements of Romanticism, Realism and Symbolism, we would be able to arrive at a new historiography of psychology. In this essay we will mainly focus upon the inherent relationship between Romanticism and the rise of experimental psychology.

SOME PRINCIPLES OF HISTORICAL PSYCHOLOGY

L'histoire ne se répète jamais
Anonymous

The Principle of Proto-Psychology

When we look at Danziger's achievements in the historiography of psychology over the last twenty years, then we can render his position as follows.

Scientific psychology is an intellectual enterprise. Research practices stand in a research tradition. The proper subjects for historical study are psychological objects. Psychological objects are 'the experimental subject', 'intelligence', 'memory', 'motivation', 'personality', 'stimulus-response', 'behavior', 'attitude', 'client' and 'therapist'. These psychological objects are socially constructed and form no part of the natural world. Intellectual interests that usually coincide with the social interests of a particular time drive the psychological scientist. There is no progress or advancement in psychological knowledge. As far as Kuhnian paradigms are concerned: there is no 'normal' science in the science of psychology. All psychological knowledge is in flux; *transition and transformation* are the

normal state of affairs. It is the task of historians of psychology to carefully map, describe, analyze and evaluate this ever-changing terrain of 'psychological reality'.

With this truly Heraclitian conception of scientific psychology before us, we may well ask what the relationship between 'psychological reality' and human reality is. If all psychological knowledge is socially constructed and if there is absolute relativity of this type of knowledge, then the very notion of 'knowledge' is in danger of dissolving in thin air. Obviously, the social-psychological constructionist approach is in some need of epistemological clarification (cf. Stegmüller, 1989; Kusch, 1999).

My answer to the challenging question just posed lies in the application of historical psychology's notion of proto-psychology as it transforms out of the life-world. Danziger's psychological objects, like personality, client and experimental subject, already exist in the life-world before they are transformed into psychological categories through research activities. In between psychological reality and the untransformed aspects of the life-world lie the mediating links of proto-psychological *settings*. Mediating links are socio-cultural practices, which bring to life particular sensitivities that may become objects of psychological inquiry. These proto-psychological settings may vary rather widely as, for example, in the psychological experiment or in psychotherapeutic situations. What we are looking for is synchronous cultural practices, which as *modus operandi* constitute the breeding ground for psychological research. This is, admittedly, a too short answer to the question of how the life-world gets into psychology (cf. Danziger, 2001; Van Hoorn, 1983). But, in general, proto-psychological sensitivities precede scientific psychology and prolong their existence in the scientific realm.

The Discontinuity of Subjectivity

In a historical psychology approach to the understanding of one century of psychology, the principle of the *discontinuity* of emotions, feelings, proper conduct and lived-through experiences should be pivotal. *Our* mental life is not identical to the emotions and experiences of our ancestors, immediate or farther removed in time. In the surrounding life-world, a person in daily life occupies what may be dubbed a 'mental space'. Mental space refers to the sum total of an individual's emotions, cognitions, and inter-subjective experiences. The individual's mental space is closely intertwined with a shared particular *mindscape*, as will be worked out in more detail.

A few examples that could illustrate the discontinuity of contemporary subjectivity itself must suffice here.

My first example is concerned with the reaction of parents to the loss of a young child. When Plutarch (1st century A.D.) and his wife lose their very young daughter Timoxena, he abundantly praises his spouse for her dignified

behavior. He is very happy to see her distancing herself from the mourning *behavior* of the women surrounding her "...the visits of silly women and their cries and lamentations" (in Lopate, 1995, p. 20). Since there is no inner 'psychological' life there are no emotions and no tears are shed.

When Ben Johnson and Joost van den Vondel (17th century) lose their very young sons, they immediately take recourse in print to providence and console themselves by writing that their children from now on will have a very good time in Heaven. Eternal glory precedes their fathers' brief period of grief.

When modern parents lose a child they are advised to go into group therapy. Both their companions in adversity and the group's professional psychologist will see to it that they properly pass through the several stages of mourning, from explicit denial to acceptance and psychological working-through of their irreplaceable loss.

As this too short example indicates, in remote times when there was no inner 'psychic' life, dignified individual behavior was the norm. In our time, with its abundance of inner feelings and emotions, the individual has to share collective mourning to be healed.

However, we need not go back to earlier centuries to find a challenging and convincing example that can convey the reality and meaningfulness of *our* changing subjectivity itself. In the *Three Essays* (1905) Freud states that a young man, who empathetically kisses the lips of a beautiful girl, will most likely use her little toothbrush only with *disgust.*

A final example. In dealing with the conceptualization of a gender identity for woman and man, Freud first assigns a *homologous* function and structure to the clitoris of the little girl. The sexual pleasure that derives from infantile mas- turbation is male in nature for both sexes. During puberty the young woman has to give up her male sexuality as far as the erogenous zones are concerned and to switch to a passive form of being prepared to receive the phallus in the entrance of her sheath. There follows one of the most remarkable metaphors of the expe- rience of female orgasm ever conceived. Adult female orgasm consists of vaginal stimulation that is set to fire by the *accidental* stimulation of the clitoris: "When the clitoris during the act of sex is stimulated she conducts this *irritability* to the neighboring female parts, *just like a bundle of pinewood is used to set the harder woodpile to fire*" (1905, p. 63, italics added). In simple language: Freud thinks that adult women can experience only vaginal orgasms. Modern women purport to know better than Freud that the lived-through orgasm is 'always' clitoral in nature. Freud's standpoint is the more remarkable because he as an experienced profes- sional *neurologist* should have known that the clitoris has thousands of sensory nerves, while the entrance of the vagina is almost deprived of them. One possible conclusion could be that Freud's notably prejudiced 'insights' are determined by scientific and extra-scientific, i.e. socio-cultural notions at the same time. In this case his assertion that libido is masculine in nature belongs to extra-scientific opin- ions. But the wording of the metaphor betrays his real prejudice: the irritability

of the clitoris has to be transferred to the entrance of the vagina, to facilitate the penetration of the penis. Sex, obviously, is a male matter; women come second or not at all.

Synchronicity in the Life-World: the Concept of Mindscape

Cultural and social history, which form the backbone of historical psychology, are complex matters. The history of psychology deals with the diachronous developments of practical and theoretical psychology, from approximately the end of the 19th century onwards. However, among all diachrony and discontinuity there is synchrony and in synchrony we may discover the existence of particular *mindscapes* that entail specific subjectivities.

Let us take European Romanticism as a prime example, because here we find the breeding ground of the contemporary inner self that has become the stuff of which the greater part of popular psychology was made in the 20th century In Romanticism we discover particular mindscapes that at the level of the life-world are characterized by a set of common characteristics. Here the synchronicity in the life-world points to the importance of dreams and unconscious psychical processes, the supernatural and transcendent, the longing for the infinite, sexuality and death, multiple personality and a new conception of the body. Together these ingredients make up a certain *Lebensgefühl* that was so dear to all true romanticists. The kernel of this romantic mindscape, is the overall emphasis on the importance of individual feelings and emotions. In 1808 Faust announced the program for the contents of both Wundt's experimental psychology and the pop psychology of the 20th century: "Feeling is everything". Passions as *stylized* motions of the soul that are closely intertwined with the notion of the four bodily temperaments, lasted till far into the 19th century The newly risen emotions became a subject of study from mid-19th century on. In between lies High Romanticism in which individual feelings became the new gospel of the age. After centuries of emphasis put on rational *man*, the romanticists, following Rousseau, placed feelings and emotions at the center of human existence. In thus doing they gave form and content to *das Psychische*, which had already sprung from the hearts and minds of their fellow human beings a few decades earlier. Here, at the *crossroads* of late Enlightenment and early Romanticism we should look for the appearance of proto-psychological settings (Eckardt et al., 2001). For the first time in social history women were also endowed with reason and men were partly gifted with emotions and feelings. A sharper differentiation of the sexes sets in and thus the issue of *gender* becomes a key point. From here on the *feminization* of cultural practices significantly increases. In a nutshell I take it that emotions on their way to transformation into proto-psychological settings first joined the scientific realm via. Darwin's pioneering work and then made a comfortable landing in psychoanalysis in which they became prime suspects for repression in traumatic (wounding) situations. Emotions presuppose

the existence of an *inner space*; passions—as their etymology indicates—befall people and are not necessarily linked to inner space (cf. Montagu, 1994; Paris, 2001). Kant had categorically maintained that psychology could never become a natural science. Quantitative measurement cannot be applied to the phenomena of the inner sense, he proclaimed. Apparently so, and especially in comparison to Wundt's pioneering work, it all comes down to our valuation of *immediate experience* (Van Rappard, this volume). Kant, as a good Cartesian, thought that psychology was about mediated experiences (introspection = retrospection of the inner sense). Kant did not perceive that under his very eyes parts of the eternal and immutable soul gave way to the temporalization of psychical processes, that is to say to the birth of the modern self (Verhave & Van Hoorn, 1984). Wundt tried hard to counter Kant's intellectualistic conception of 'psychology'. Through precisely using the very tools of natural science, he hoped to show that psychology is about *immediate* experience. *Seemingly* simple reaction-time measurements marked the birth of experimental psychology. But, unsolved question, did we eventually get a science of feelings and emotions?

Romantic mindscapes form the heart and hinge of a world-view. We may well wonder whether in cultural history this romantic world-view was the first encompassing outlook on people's comings and goings. This *holistic* world-view *materializes* in literature, philosophy, painting, architecture, music and sculpture. For this very reason its diverse tangible manifestations are properly called a mindscape in analogy to landscape, seascape, beachscape and cityscape, which are material parts of the life-world as shaped by the continuous activities of the creative mind, in which memories always play their salient roles.

Last, not least, a notable romantic mindscape materialized in Wundt's experimental psychology. It is Goethe's simile of *psychical chemistry* as it was reshaped in Wundt's principle of *creative synthesis* to which we now turn.

In chemistry we deal with earth and minerals, with salts and acids. These natural elements seek and modify each other and together they form new entities. The same holds for human relations. Simple feelings and emotions, once stirred up, may form new connections and so may the persons who experience them. A chemical simile is fully appropriate here, because "...there is only one Nature", as Goethe remarked. Wundt also works with psychical elements. These consist of elementary psychical parts, the so-called pure sensations and simple feelings. Through systematic experimentation Wundt opened up the possibility of a literally endless multiplication of total feelings. As Boring already concluded: "Wundt (was) free to employ the newly created feelings for many purposes" (Boring, 1957, p. 330). The fundamental characteristic of the psychical events is '...that the product of whatever number of elements is more than the mere sum of the elements; it is a new simply incomparable creation' (paraphrased and translated from Wundt, 1911, p. 755). This, in a nutshell, comprises Wundt's principle of *creative synthesis*. And what could already be read in Goethe? "You have to see

these elements actively working before your eyes ... how they seek each other, and then, reappear in a new, unexpected Gestalt" (1809, p. 396; see Van Hoorn, 2000).

Wundt's psychical elements and the always-new Gestalten they bring forth is a very interesting case of psychological thinking that is closely related to a particular romantic mindscape. In Goethe we have its inter-subjective literary form; in Wundt we see its scientific elaboration. Through the introduction and use of scientific apparatus—the famous brass instruments—the subjectivity of once ordinary collaborators transformed into proto-psychological sensitivities that provided the materials for objectified scientific inquiry (cf. Danziger, 1990, Ch. 10). The brass instruments, like Hipp's chronoscope, which measures time-intervals in *thousandths* of a second (sic!) could be conceived of as extensions of the human sense organs. The 'measured' psychical reaction time is an artifact resulting from the interaction of sensory-motor perceptions and the relentlessness of the instrument.

THE SYNCHRONICITY OF UNCONTEMPORARY SCIENTIFIC MOVEMENTS

The principle of the simultaneous existence of uncontemporary cultural movements forms the counter-principle of synchronicity in the life-world. Old life forms and new life forms in cultural history always exist simultaneously. In historical psychology we have breaks, interruptions, discontinuities and dialectical processes going on forever. In times of cultural crises (1770–1805; 1880–1900 and 1965–1975) old life forms and the emerging new ones heavily collide (Praz, 1933/1979; Clark, 1999). The clash of the old and the new life forms fuels the engine of the course of cultural history.

In psychology, psychoanalysis, Gestalt psychology, behaviorism and phenomenological psychology all existed side by side. Since the late sixties cognitive psychology (man-machine mindscape) and evolutionary psychology (adapting animal mindscape) develop almost independently. Each of these movements has its own mindscape, but the mindscapes as such are at odds with each other.

As an invitation to further study and elaboration I would like to call attention to the fact that both Wundt's *Elements der Völkerpsychologie* and Freud's *Totem and Taboo* appeared in 1912. Even a superficial comparison of the contents of both works shows a considerable overlap in topics. Both scholars, the one in his early eighties, the other 56, are primarily interested in 'the life of the mind of primitive peoples'. Theoretical differences, however, could not be wider apart. Wundt conceives of his endeavor as 'developmental psychology', while Freud in creating the myth of the killing of the *Urvater* takes recourse to the assumption of *unconscious collective memories* to 'explain' present day forms of neurotic behavior (Freud, 1913, pp. 157–58). In foresight we can conclude that Wundt's summary of his life's work marks the end of a psychological era, while Freud's daring generalizations

open up a new field of psycho-anthropological study (Muensterberger, 1969). As this example should make clear, the co-existence of uncontemporary scientific movements is rather the norm than the exception.

THE INVISIBILITY OF PSYCHOLOGY TODAY

When we now pause to ponder the 'present' status of scientific psychology and try to further delineate the outlines of the emerging field of historical psychology, one of the basic questions remains: what could be the course and *direction* of human history? We already have definitive answers from Jewish, Christian, Muslim, Marxist and 'progressive' eschatologies. Suffice it to sadly remark that these answers and their translations into inhumane actions have cost the lives of billions of men, women and children. And the killing goes on.

From the little that we now know about the historical psychology of the 19th and 20th centuries, we have to conclude that the present has always been invisible and hence that the future is *unpredictable.* Contrary to the grandiose stage schemes of Hegel, Marx, Comte and Freud, it must be unequivocally stated that there are no fixed historic laws. In following Karl Popper, I am talking about open societies and their fundamentalist enemies (Popper, 1966). It lies in the course of historic processes that extrapolations, which are based upon the present state of affairs, always fail because *historic events* are by their very nature unpredictable (e.g. 14th July 1789; 7th December 1941; fall of the Berlin wall in November 1989 and the 11th September 2001).

Since the present is invisible, the present status and meaning of psychology as a whole is also invisible and thus its future course is unpredictable. The cardinal vice of historians of science is to start from *some* rather arbitrary present situation in the field and then to write history backwards. Difficult as it may be, history of psychology should be written forwards with no specific idea about 'future' developments in mind. This holds for cultural history in general and it also holds for the historiography of science. It even holds for the work of one scientist-scholar. Any comparison of the work of the 'young' Wundt to the work of the later Wundt must lead to almost meaningless generalizations because young Wundt could not know what mature Wundt would come up with. No scientist working at 'psychology-in-the-making' knows at what place in social time he or she is located. The two subtitles of Danziger's major works point to a certain presentistic bias: 'Historical *origins* of psychological research' (1990), and: 'How *psychology* found its language' (1997). Obviously, the author of these outstanding contributions already knows to some extent what recent *American psychology* pertains to and from this vantagepoint he searches the past for 'origins' and 'first emerging' of psychological categories. Thus, the as yet unknown historical *zigzag* course of the 'discipline' and all the *partial* developments we have lost on the way, can hardly come into sight (for the notion of 'zigzag' history, see Husserl, 1936/1954).

I think it here almost unnecessary to reiterate that both Claude Lévi-Strauss and Michel Foucault time and again have pointed to the pitfalls in the search for 'origins'. The greater part of the published history of psychology so far has been inspired by a *quasi-evolutionary historiography* that smacks more of Lamarck than of Darwin's position. Necessarily this mutes the actual developments, which have taken place and it suggests a unity of scientific practices that never has been the case (Levi-Strauss, 1958/1967 Foucault, 1972). Admirable as a work like *Constructing the Subject* may be, to write an archeology of psychological knowledge, is a rather different matter (cf. Louw & Danziger, 2000; Foucault, 1977).

An archeology of psychological knowledge has, as a minimum, to start from the following viewpoints. The contents of the published research results have to be related to their *deep structure.* This deep structure is content-wise set in cultural movements such as Romanticism (Wundt) or Symbolism (Freud). The meta-psychological *overarching* factors in the creation of psychological knowledge are positioned in ontological, ethical and epistemological systems like idealism and voluntarism (Wundt) or materialism and determinism (Freud). *Transformation schemata* of one life-style into another, of one concept into a new conceptualization, of one accepted research strategy into a new approach, emergence of new methodologies and the disappearance of once accepted scientific view-points and life-forms, should be our historiographical objectives (Foucault, 1972, p. 131; Piaget, 1970).

The Reversal of Chronological Time

When educated Greeks talked about the future, they would say: 'What do we yet have *behind* us'? This challenging and interesting experience of time's course is still of value to the founding of historical psychology. Since the future of scientific psychology from 1879 on is invisible *at any point in time*, it should be unknown to the historians of psychology who analyze the course of 20th century psychology. From this it follows that the future of scientific psychology should always be regarded as lying *behind* us, whereas its past, as embedded in proto-psychological settings, is known and therefore stretches out *before* our investigating minds and eyes.

Let us take an example from Danziger to make clear which historiographical consequences could be drawn here. Early in his career as a historian of psychology Danziger published a now famous article entitled "The positivist repudiation of Wundt" (1979; see also Danziger, 1990, Ch. 3). The main argument of this paper is that some of Wundt's pupils and his immediate successors turned 'positivistic' under the influence of Mach's philosophy of science. This positivistic turn in theoretical psychology is one of the main reasons for the demise of the Wundtian system at the beginning of the 20th century.

What Danziger has accomplished here is the result of a comparison of Wundt's system with *later* developments in theoretical psychology, namely, the systematic

experimental study of higher thought processes in the so-called Würzburg School (Kusch, 1999). However, Wundt could not know what turns theoretical psychology would take just after 1900. And we historians writing about Wundt, are well advised to better take the same position. We must fear then that we have, as yet, not got a full picture of Wundt's position in this crucial matter.

If we turn our attention to the past of Wundt's holistic system, then it is possible to show that it comprises the last stage of a psychological, anthropological and philosophical position that for the greater part is over and done with before 1900! The 'positivist turn' of some of his pupils could be just one example of this, but it is, internalistically seen, no sufficient explanation for the 'system's' demise. What could have been done then, is to carefully study the psychological procedures in Helmholtz's, Donders's and Fechner's experimental approaches and thus to make clear what Wundt's true contributions and arising 'failures' have been.

Yet, the main reason for the dissolution of Wundt's type of theoretical psychology lies in the outside world. It is the rapid emergence of the fields of psychological practice that has truly prevented the construction of *one* unified theoretical psychology as envisaged by Wundt.

However, there probably survives more of Wundt's systematic thinking in e.g. Gestalt psychology than its hard-nosed parishioners would ever like to admit. As Brock has put it in a daring formulation: "The fundamental theoretical issues that occupied Wundt (. . .) have never been resolved and are still of contemporary relevance" (Brock, 2000, p. 6; Van Rappard, this volume).

The Necessity of Periodization and Comparative Study

Over and again Danziger has stressed the relativity of all psychological knowledge. I have added the idea that all psychological knowledge is embedded in proto-psychological settings. Here we are in danger to become so relativistic that we would overlook the *typical* developments of 20th century psychology.

In his excellent chapter "On the threshold of the new psychology: Situating Wundt and James" (1980), Danziger maintains that the psychological systems of these two 'founding fathers' of scientific psychology should be seen as *transitional periods*. This is a challenging thought and it is worthwhile to follow up on its historiographical and epistemological consequences. Wundt and James are positioned between the 'old' psychology (unscientific) and the 'new' psychology that is distinctly 'scientific'; hence: 'threshold'.

Let us shortly take a look at the Wundtian System to find out whether this position can be fully upheld.

The *contents* of Wundt's experimental psychology stem from the last phase of German Romanticism: feelings and emotions. The *methodology* fits into the 'positivism' of the natural sciences: *physiological* psychology. The overarching metaphysiology is intrinsically linked to German idealism: voluntarism and values as the key ingredients. In the light of the principle of the reversal of chronological

time I would like to suggest now considering the Wundtian holistic system as the last stage of German idealism that was clearly over and done with by 1900. And thus, what is the status of Wundt's type of psychological knowledge? Here we have to combine the problematic of the archeology of psychological knowledge and the necessity of periodization. The actual experiments in Wundt's laboratory are related to the deep and overarching structures of romanticism, positivism and Idealism. Both romanticism and idealism had had their days by 1900. And thus, paradoxically, it should have been positivism that would have paved the way for Wundt's triumphal march into 20th century psychology. It did not, as Danziger has amply made clear. The deep structure and the overarching value system have definitely hindered the continuation of Wundt's type of experimental psychology. To come to that conclusion there is no real need for a comparison to later systematic positions in theoretical psychology.

As an invitation to further discussion I would like to suggest constructing a periodization of the history of scientific psychology. Let us consider Wundt's system as belonging to Period 1. Here is some solid psychological knowledge available. Especially when we also take the monumental *Völkerpsychologie* into consideration, then there is still a lot of interesting work to be done. To the same period pertain the emergence of functional psychology (James), of depth psychology and psychoanalysis and of phenomenological psychology in Brentano and Husserl. Instead of thinking that only positivism will lead the way—the scientistic fallacy, the use of the principle of the synchronicity of uncontemporanous scientific movements here shows us again that scientific psychology has been polymorphous from the very start and has stayed so during the 20th century.

By general agreement, the period of 1880–1910 is called 'La belle époque'. That is our Period 1 for the history of scientific psychology. Unlimited hopes were expressed for the future century of psychology. Period 2 comprises the significance of World War I for the definitive establishment of the fields of psychological practice, both in Europe and in America. Period 3 pertains to the rise of the grand systems and the significance of World War II for the emergence of the field of clinical psychology. Fourth and last is the period up till the mid 'seventies, during which practical and theoretical psychology split up into an unrecognizable number of highly specialized sub-disciplines. Modesty befits the historian of psychology. Even if one finds 'the invisibility of the present' a too far-fetched conception, then the utterly complex present situation in practical and theoretical psychology is a sufficient reason to banish presentism forever.

DANZIGER'S CRITICAL HISTORIOGRAPHY OF PSYCHOLOGY

Somewhat earlier we have described Danziger's views on the historiography of psychology in a nutshell. One of the main conclusions was that the proper subjects for historical study are the 'psychological objects'. We may infer that

he considers himself to be a *critical* historian of psychology. This is to say that, among other things, he wants the history of psychology to serve as admonisher with regard to current work in theoretical psychology.

Right from the start, Danziger has rejected all forms of naturalism, objectivism, scientism, essentialism and progressivism. He *problematically* contends that scientific psychology does not find its objects and experimental subjects in 'the natural world'. However, since the findings of quantum mechanics have become common knowledge, we do not assume that there is such a thing as 'the natural world'. "We do not study nature as such, but the problematized parts of nature" (Heisenberg, 1955/2000). Neither physicists nor psychologists find their objects in the natural world, but both belief in hard matter and real people. In a sense, all sciences are human sciences.

Danziger's position is, as already described earlier, that psychological objects like 'intelligence', 'attitudes', 'memory' or 'personality' are the products of peculiar forms of social-psychological construction, which could not have existed before 1850. Danziger even maintains that notions like 'depression', 'the self' and 'emotion' are of recent origin, "often being younger than the discipline itself". Somewhat rhetorically, Richards writes: ". . . nobody prior to Freud had an Oedipus complex, nobody before Pavlov and Watson was ever 'conditioned' and nobody before *c*. 1914 had a high IQ" (Richards, 1996, p. 7). I think that these statements are rather problematic. Without sound argumentation they do not make much sense. For brevity's sake, let me comment as follows.

Whatever constructs scientific psychologists make up, as a rule their 'origins' are situated in proto-psychological settings and the life-world, and otherwise they have no resonance. It is precisely here where scientific psychology and historical psychology meet and can be seen as complementary parts of the proper study of human comings and goings

From an overall point of view I take it that Danziger means to purport that already existing characteristics of human beings were *partly* transformed into *psychological objects* during the late 19th and early 20th centuries, concomitant to the establishment of psychology as a science. My position would fit into Danziger's view of the historically determined *emergence* of the psychological objects themselves as the products of changing human relationships. In short, the historicity of human subjectivity—which exists partly within and partly outside scientific endeavors and which is connected to both of them via the life-world—should be taken for granted. Danziger's type of social constructionism falls largely within the framework of recent intellectual history with a social bent. It is mainly intellectual interests that coincide with the social interests of a particular time period that drive the psychological scientist.

I have little reservation to propound that proto-psychological subjectivities as such only exist from the end of the 19th century on. Here the historical order of the issues at stake is as follows. The linguistic subject-predicate structure is as old as the classical languages. In Descartes' anthropology we get the

very sharp distinction between mind and matter. 'Subjective' in the sense of belonging to the conscious mind, dates from the beginning of the 18th century *Subjectivity*, as might be expected, has as its dates the 1820s, that is to say the heydays of Romanticism. From then on subjectivity means individuality and personality. This order of things shows that the self, individuality and personality already existed quite sometime *before* scientific psychology partly turned them into psychological objects through making use of specific proto-psychological settings.

As far as the *structure* of psychological knowledge is concerned, Danziger focuses on the problem-solving context in which research takes place. Then he distinguishes between the problems to be solved and the *problematic* underlying them, to proceed to the notion of the *collective subject.* With respect to research this can take the form of active participation in an intellectual tradition or in a social grouping like a laboratory situation. The latter is embedded in a wider social context, the life-world, in which knowledge-constituting interests (Habermas, 1972) are always at stake. Danziger has not thoroughly explored this neo-Marxist stance, because that would inevitably have led to the conclusion that a critical history of psychology is not *primarily* about the history of psychological objects or of the fads and foibles of the psychological experiment, subjects that are Danziger's strong research issues. 20th century psychology is mainly about the development of the fields of psychological practice. When we focus upon the psychology of labor, education, health and individual differences (testing), e.g., then the knowledge-constituting interests become almost all too obvious (cf. Van Hoorn & Verhave, 1977; Danziger, 1990, Ch. 7). Danziger's contention that the intellectual interests of the scientific psychologists usually coincide with the social interests of a particular time period, is too general. It ignores the conflict-ridden nature of social history. If we take conflicting social interests into account, then we would have to describe and ethically value how much of 20th century psychology is *Herrschaftswissen* (power knowledge) and how much of it belongs to *Bildungswissen* (educational knowledge, according to Max Scheler). Here we hit upon the Achilles heel of Danziger's work so far. To recognize that all psychological knowledge is constructed in a *social-psychological* context is one thing. However, this *context of discovery* situation does tell us almost nothing about the *natures* of psychological *knowledge.* The development of 20th century psychology cannot fully be understood only by walking social-psychological avenues. Reference points that are situated outside psychology itself have to be brought to the stage. As Huizinga has put it so succinctly: "History is the way in which a culture *accounts* for its past" (Huizinga, 1929/1950, p. 100, italics in original, translation WvH). In the same vein a critical historiography of psychology has to provide tools for the accounting of psychology's past. The notion of culture as coordinated action, as we have already indicated, has a decisive role to play here. As far as the *primacy* of the fields of practice is concerned, the very notion of praxis/practice itself still deserves further analysis (Louw & Danziger, 2000).

HISTORY OF PSYCHOLOGY AS A WAY
TO SELF-UNDERSTANDING

"And then I start thinking about myself, and what good can that bring?"
Helen Hunt in *As good as it gets*

Danziger has eloquently set forth that his non-critical (non-discerning?) adversaries mainly use history as self-serving, celebratory, self-congratulating and as a legitimization of the present 'progressive' state of the art. I gladly share this position.

Somewhat to my surprise, Danziger also thinks that the results of historical study can improve the present status in theoretical psychology. "A study of the origins and backgrounds of basic psychological categories can help first of all, to question one's questions and, secondly, lead to *better* questions" (Brock, 1995, p. 16; italics added). Let me comment as follows. First, as long as we take each other seriously, any good scholar-scientist has asked good questions. Helmholtz and Wundt had their good questions and so had Freud and Skinner.

Second, when we deal with past human experiences and behavior, we are confronted with differences and resemblances. The social past can only be understood through analogies. I can partly understand what people in the past were feeling and experiencing and partly they will forever remain strangers to me. The same holds for myself and my past, which stretch out before my eyes. Partly I know myself, but parts of myself look strange to me (I am not saying that parts of myself are unknown to me, as Freud loves to maintain, because I cannot know what I know not).

In this connection I again suggest to let the history of theoretical psychology proper deal with its development from Period 1 on. All the work, which Danziger has done on Wundt, for example, belongs to this time frame and should be considered as such. The dangers of anachronism are always with us. Wundt's weighty volumes on logic, ethics and experimental psychology tell us much more about the newly arisen *sensitivities* of the psychological subject as conceived of by one great psychologist-cum-philosopher, than that they would really help us to better formulate questions for 21st century. theoretical psychology. To read Wundt or Durkheim widely and deeply on social or cultural anthropology will improve my self-understanding, not my present psychological endeavors. This is to say that in dealing with Wundt's works I need to be as critical and discerning as possible. The fruit of my historical efforts will be the discovery of the *true differences* of his psychology and mine. All historical study furthers self-knowledge (Van Rappard, 1979). A serious study of the past must lead to a better understanding of my own life. There is a discontinuous relationship between past and *recent* scientific and practical activities. The world of the historian of psychology is not the world of

the scientific psychologist. Without much substantiation it should be clear that mainstream psychologists are not in the least interested in applying the 'great findings of the past' to their present endeavors. Maybe they are more right in taking this position than would appear at first sight. Since the history of psychology is polymorphous and discontinuous through and through, how could they fruitfully make use of the bygone 'findings' of the past? Here Danziger's self-imposed task seems Sisyphus labor to me.

Another reason why Danziger's cleansing approach towards the development of a more purified theoretical psychology has no firm ground to stand on is that the 'subjectivity of the subject' itself will again already have changed *or disappeared (!)* by the time he and his followers have finished their methodological mopping-up job. Here the social-constructionist approach will necessarily bite into its own tail.

Social life itself is always decades ahead of psychologists who study life after the facts. This phenomenon we might call the *Nacheilungsparadox* (the paradox of the lagging behind of theoretical psychology with regard to the speed of social history). After WW II theoretical psychology looks more and more like a ticker-tape factory. No serious historian of psychology can pretend to even possess a bird's eye overview of what actually is going on. Endless specialization defies the very idea of a unified theoretical and practical psychology. This viewpoint nicely fits into the characteristic of the fourth period of the history of scientific psychology, that is to say the disappearance of the grand systems and the ongoing proliferation of the fields of practice. 'The psychology of health' forms only one example of the abyss that separates well-proven behavior modification techniques and desirable ways of living on which there is no consensus at all.

Although recent psychology is truly invisible and hence the future of the science of the subject is unpredictable, my best guess is that one day people will wake up and will discover to their own amazement that they do not need a science like psychology. Then Barthes' epigram will have reached full fruition.

HISTORICIZING THE SUBJECT

"There is no other reality than the one that we carry in our memory. In between us and nothingness strands our power of recollection, a problematic and fragile refuge"
Klaus Mann

A critical writing of the history of 20th century psychology must rest upon a well thought-out historiography of the discipline as a practical-applied and theoretical enterprise. The major works of Kurt Danziger in the theoretical field stand as solid reference beacons. Danziger has amply made clear that psychological knowledge is closely knit to specific social-psychological situations. The most salient example of this is the psychological experiment, in which subjects and

experimenter share common proto-psychological sensitivities that are rooted in the life-world. This world of unrecorded and untransformed experiences is part of a social-cultural situation. Thus, the history of psychology as such has to be intertwined with the emerging discipline of historic (cultural) psychology. Cultural history itself is a succession of the coordinated actions of specific generations and of particular aesthetic movements without improvement or progress. What we can hope for is universal democracy and universal emancipation, that is a freeing from mental and bodily slavery. If scientific psychology can contribute to the approaching of that utopian goal then its coming into existence has not been in vain.

The history of the human sciences is a succession of social practices and theoretical endeavors without advancement or accumulation. Only if we widen the accepted social construction of psychological *knowledge* to the ever-changing *cultural actualities* in which emotions, cognitions and feelings are *experienced* as coordinated actions in the shared life-world, the very subjectivity of the human subject will appear as a genuine recent, i.e. a *modern* phenomenon.

ACKNOWLEDGMENTS

SELECTED REFERENCES

Boring, E.G. (1957). *A history of experimental psychology.* New York: Appleton-Century-Crofts.

Brock, A. (1995). An interview with Kurt Danziger. *History and Philosophy of Psychology Bulletin, 7*, 10–31.

Brock, A. (2000, August). *Danziger on Wundt*. Danziger Symposium. Paper presented at the 19th annual conference of the ESHSS, Berlin.

Clark, T. (1999). *Farewell to and idea. Episodes from a history of Modernism*. New Haven: Yale University Press.

Danziger, K. (1979). The positivist repudiation of Wundt. *Journal of the History of the Behavioral Sciences, 15*, 205–230.

Danziger, K. (1980). On the threshold of the new psychology: Situating Wundt and James. In W.G. Bringmann & R.D. Tweney (Eds.). *Wundt Studies* (pp. 363–379). Toronto: Hogrefe.

Danziger, K. (1990). *Constructing the Subject—Historical origins of psychological research*. Cambridge: Cambridge University Press.

Danziger, K. (1997). *Naming the Mind—How psychology found its language*. London: Sage.

Danziger, K. (2001). Sealing off the discipline—Wilhelm Wundt and the psychology of memory. In C.D. Green, M. Shore, & T. Teo (Eds.), *Psychological thought in the nineteenth century—the*

transition from philosophy to science and the challenges of uncertainty, (pp. 45–62). Washington, D.C.: American Psychological Association.

Delphy, C. & Chaperon, S. (Eds.) (2002). *Cinquantenaire du Deuxième sexe.* Paris: Syllepse.

Eckardt, G., John, M., van Zan Twijk, T. & Ziche, P. (Eds.) (2001). *Anthropologie und empirische Psychologie um 1800.* Köln: Böhlau.

Foucault, M. (1972). *The archeology of knowledge.* New York: Pantheon Books.

Foucault, M. (1977). *Language, counter-memory, practice.* D.F. Bouchard (Ed.). Oxford: Basil Blackwell.

Freud, S. (1905). *Drei Abhandlungen zur Sexualtheorie*. Wien: Franz Deuticke.

Freud, S. (1913). *Totem and Taboo.* Standard Ed. Vol. 13. London: Hogarth.

Galton, Fr. (1879). Psychometric experiments. *Brain, 2*, 149–162.

Giorgi, A. (1970). *Psychology as a human science. A phenomenologically based approach*. New York: Harper.

Goethe, J.W. von (1809). *Die Wahlverwandtschaften*. In *Werke*, part 2. Salzburg: Caesar Verlag.

Habermas, J. (1972). *Knowledge and human interests.* London: Routledge.

Heisenberg, W. (1955; 2000). *La nature dans la physique contemporaine.* Introd. par Catherine Chevalley. Paris: Gallimard.

Huizinga, J. (1929/1950). *Over een definitie van het begrip geschiedenis*, VII. Haarlem: H.D. Tjeenk Willink & Zoon.

Husserl, Edm. (1936/1954). *Die Krisis der europäischen Wissenschaften.* Husserliana, VI. The Hague: Nijhoff.

Kusch, M. (1999). *Psychological knowledge. A social history and philosophy*. London: Routledge.

Le Goff, J. (1977/2001). *Pour un autre Moyen Age.* Paris: Gallimard.

Lévi-Strauss, Cl. (1958; 1967). *Structural anthropology.* Garden City, N.Y.: Doubleday.

Lopate, P. (Ed.) (1995). *The art of the personal essay.* New York: Doubleday.

Louw, J. & Danziger, K. (2000). Psychological practices and ideology. *Psychologie & Maatschappij, 90*, 50–61.

Mak, G. (1999). *De eeuw van mijn vader.* Amsterdam: Atlas.

Merleau-Ponty, M. (1945). *Phénomenologie de la perception.* Paris: Gallimard.

Merleau-Ponty, M. (1966). *Sens et non-sens.* Paris: Nagel.

Montagu, M. (1994). *The expression of the passions.* New Haven & London: Yale University Press.

Muensterberger, W. (Ed.) (1969). *Man and his culture. Psychoanalytic anthropology after 'Totem and Taboo'*. London: Rapp & Whiting.

Paris (2001). *Figures de la passion.* Exhibition Catalogue 23–10–2001–20–1–2002. Paris: Réunion der Musées Nutionavx.

Piaget, J. (1970). *Le structuralisme.* Paris: Presses Universitaires de France.

Popper, K. (1966). *The open society and its enemies.* London: Routledge.

Praz, M. (1930; 1979). *The romantic agony.* Oxford: Oxford University Press.

Richards, G. (1996). *Putting psychology in its place.* London: Routledge.

Schopenhauer, A. (1819). *The world as will and representation.* New York: Dover.

Stegmüller, W. (1989). *Hauptströmungen der Gegenwartsphilosophie.* Band I-IV. Stuttgart: Kröner.

Van den Berg, J.H. (1956/1961). *The changing nature of man.* New York: Norton.

Van Hoorn, W. (1983). The cultural context of psychoanalysis. In G. Bittner (Ed.), *Festschrifft für Ludwig Pongratz*, (pp. 335–350). Toronto: Hogrefe.

Van Hoorn, W. (2002). Goethes Gleichnisrede der psychischen Chemie als romantischer Hintergrund von Wundts experimenteller Psychologie. *Psychologie und Geschichte, 10*, 233–246.

Van Hoorn, W. & Verhave, Th. (1977). Socio-economic factors and the roots of American psychology: 1865–1914. In R. Rieber & K. Salzinger (Eds.). *The roots of American psychology: Historical*

influences and implications for the future, (pp. 203–221). New York: Annals of the New York Academy of sciences, 291.

Van Rappard, H. (1979). *Psychology as self-knowledge.* Assen: Van Gorcum.

Verhave, Th. & Van Hoorn, W. (1984). The temporalization of the self. In K. Gergen & M. Gergen (Eds.). *Historical social psychology*, (pp. 325–345). Hillsdale: Erlbaum.

Wundt, W. (1911). *Grundzüge der physiologischen Psychologie*. Sechste Aufl. Dritter Band. Leipzig: Wilhelm Engelmann.

Wundt, W. (1912). *Elemente der Völkerpsychologie. Grundlinien einer psychologischen Entwicklungsgeschichte der Menscheit*. Leipzig: Alfred Kröner Verlag.

DE-CENTERING WESTERN PERSPECTIVES

PSYCHOLOGY AND THE DISCIPLINARY ORDER IN THE FIRST AND THIRD WORLD

Irmingard Staeuble

In the opening chapter of *Naming the Mind* (1997) Kurt Danziger tells the story of his unsettling first encounter with an 'exotic' double of his discipline. He had just resumed teaching psychology at an Indonesian university when he discovered that an Indonesian colleague already taught a course that translated into 'science of the soul.' He thus hastened to suggest a joint seminar but when they began to discuss topics for the seminar "the problem began. There seemed to be virtually no topics that were identified as such both in his and in my psychology" (p. 1). Topics like motivation, intelligence or learning did not make much sense to the Indonesian colleague who in turn suggested topics based on the local syncretistic philosophical tradition which were unacceptable to the young experimental psychologist. The encounter took place in the early years of post-Independence Indonesia, about the same time as Clifford Geertz conducted his ethnographic studies of religion as a cultural system portraying a 'philosophical-minded' Javanese population. The Indonesian government had started to import Western psychology to promote modernizing reforms and Danziger was their employee (Brock, 1994). Reflecting on the intriguing encounter Danziger observed: "It was clearly possible to carve up the field of psychological phenomena in very different ways and still end up with a set of concepts that seemed quite natural, given the appropriate cultural context. Moreover, these different sets of concepts could each make perfect practical sense, if one was allowed to choose one's practices. What did that imply for the objectivity

of the categories with which Western psychology operated?" (1997, pp. 2–3). The question remained with him, although he first addressed other major themes in the history of psychology—its social origins (1979) and the social construction of the 'subject' (1990)—before returning to the puzzling status of the categories that structure Western psychological discourse. Scrutinizing their universalistic claims he compellingly demonstrated that Western psychology is "a cultural construction with specific historical roots" (1997, p. 181).

Drawing on Danziger's deconstruction of the linear textbook historiography from the imperial view of 'center and periphery,' I discuss some of the problems involved in the large-scale export of psychology as a discipline and profession to the non-Western world. The features of psychological discourse analyzed by Danziger were characteristic of this U.S.-made product at the height of its export. I briefly summarize these characteristics and provide the political and intellectual context in which the product 'behavioral science'—with its universalistic presumptions—was exported to the non-Western world. I then turn to the operations of the international knowledge network and the responses of Third World psychologists to the imported product. Evidence of many voices from many locales is to emphasize the difficulties of identifying the problems with the imported product. In some quarters, the debate concerning the 'indigenization' of psychology focused on the theoretical question of how to develop concepts more in tune with local traditions of human self-understanding. I outline the scope of this debate and discuss some problems involved in attempts at reworking the foreign product. The expectation of some Third World psychologists that alliances with other social sciences might substantially improve the chances of making the discipline more socially relevant opens a wider problem. Drawing on other social sciences would only multiply the difficulties as they, like psychology, are each components of a disciplinary order representing a specific Western construction of reality. Expanding on some of Danziger's observations, I argue that in order to deconstruct the hegemonic claims of psychology one must reflect upon the historicity of both the disciplinary order and modernity. This issue has been brought to the fore by postcolonial critics concerned with decolonizing the mind, theory and methodology. I discuss some contributions to stress the importance of the polycentric approach to the history of psychology and modernity advanced by Danziger.

PSYCHOLOGY AS A CULTURAL AND SOCIAL CONSTRUCTION

According to positivist wisdom, psychologists developing theories about motivation, personality, intelligence or behavior and historians of psychology writing histories of 'motivational' or 'personality' psychology take their domains as natural kinds which, taken together, reflect the universal structure of 'the psychological.'

Danziger's post-empiricist subversion of this understanding is based on an examination of "the presuppositions about our subject matter that are implied in the categories we use to define the objects of our research and to express our empirical findings" (1997, p. 8). Unraveling the various historical layers of discursive formations that have provided the components of modern psychological discourse, he has made visible a cultural construction the conceptual basis of which does not date back far beyond eighteenth century.

The origins of this conceptual basis in the socioeconomic 'Great Transformation' (Polanyi, 1944) towards commercial society in Europe is of special importance to my discussion below. The emergence of industrial capitalism gave rise to a new view of society as constituted by individual actors in marketplaces turning previous views of the relationship of society and economy upside down. The parallel 'great transformation' in British moral philosophy analyzed by Danziger consisted in the reconceptualization of questions relating to human experience and conduct as questions pertaining to a 'psychological' domain. This transformation reflected aspects of a changing self-image of human actors brought about by 'commercial society.' This entailed a "new conception of the relationship between human persons and their actions" that separated agents and their actions, so that 'motives' were needed to mediate them (Danziger, 1997, p. 46). With the new view of self-interested calculated action as "the 'natural' form of human action in general" (p. 46), 'economic man' became the model for accounts of the intelligibility of human persons and their actions. The roots of the influential binary opposition of male 'rationality' and the 'irrationality' of women, children, and savages are in this model.

A further layer of psychological discourse added in the nineteenth century was based on the adoption of physiological categories like organism, stimulation, and energy. As argued by Danziger, this biological heritage "weighs heavily on pervasive categories . . . like behaviour and learning" endorsing "an essentially biological understanding of human action and human capability" (1997, p. 182). Yet the decisive change in the establishment of the meaning of the modern psychological categories came with its institutionalization as a discipline and its professionalization. The authority of psychologists as experts depended on the maintenance of "a delicate balance between the ideal of a universalistic and uninvolved science and accommodation to the requirements of local sectoral interests" (p. 182). Mediated by specific technologies, such as the investigative practices analyzed in *Constructing the Subject* (Danziger, 1990), new categories derived from local social practices (e.g., education, management) were now infused with a universal biological meaning. For instance, the twentieth century dictum that all action or behavior is motivated gained its currency in practical contexts of social influence and control where the question was how to 'motivate' people to work harder or to display other socially valued behavior. The psychology of 'motivation' reified a set of historically contingent assumptions about the intrapersonal forces that

account for the 'why' of human behavior and elevated them to the status of universal human needs. The most prominently discussed needs, for 'achievement' and for 'self-realization,' show the cultural norms implied in the category of 'motivation' (cf. Danziger, 1997, pp. 110–123). Small wonder that Danziger's Javanese colleague did not see much sense in this category.

By the mid-twentieth century the American version of this cultural and social construction of psychology had come to dominate the discipline. Its implied notion of the individual person "as a bounded, unique, more or less integrated motivational and cognitive universe" (Geertz, 1983, p. 59) increasingly impacted self-understanding within Western culture. The anthropologist's caveat that this notion is "a rather peculiar idea within the context of the world's cultures" (p. 59) went largely unnoticed by psychologists.

PROMOTING PSYCHOLOGY IN THE NON-WESTERN WORLD

Modern psychology in Europe was still in the making when, at the turn of the twentieth century, it began to move west (the U.S., Canada and Latin America), east (to Russia, Asia, and Oceania), and south (Africa). Colonialism provided the main context, although this may have been noticed less in the colonial settlements of the Americas, Australia, and New Zealand than in the European-dominated colonies such as India or the Philippines where American colonial rule replaced Spanish domination. Documenting the 'silk road' to the East, Alison Turtle (1987) has shown that in British colonies such as India, Australia, and New Zealand, philosophy departments provided the gateway for psychology's entry, with the notable difference that Australia and New Zealand represented "the distinctive situation of an extension of imperial science into a cultural vacuum, as the Australian aborigines and New Zealand Maoris were sufficiently few in number to exert any influence on such matters" (1987, p. 5). Yet, like sub-Saharan Africans, Aborigines and Maori had been subjected to psychological observation in the context of anthropological research long before the first departments of psychology were established on this terrain.

As the focus of this chapter is on the post-World War II period, I can but briefly mention that historical investigation into the diversity of colonial conditions which provided an entry for psychology would obviously have to take a broader view of the complex implication of the sciences in colonialism. For instance, geography, anthropology, biology and eugenics found a role in marking and mapping places and people, in classifying non-European social worlds, and in the invention of a racialized order of human resources (Dirks, 1992; Stocking, 1991; Jahoda, 1999). Scientific racism pervaded the interpretation of human differences in terms of immutable 'race' characteristics long before psychological testing devices were used to sort the 'primitives' according to their mental capacities (Gould, 1981;

Richards, 1997; Probst, 1992). In the collective memory of the colonized, to which I will return later, these encroachments were to have a lasting effect.

The post-war period of large-scale export of American psychology to the non-Western world was marked by the dismantling of the European empires and the Cold War. Within two decades colonial struggles for independence resulted in the formation of some sixty-five new nations, since termed the Third World. The Cold War provided the pretense for a strategic view of these nations in terms of cooption or subversion. Although primarily economic, American expansionism also involved culture in its strong support for the establishment of UNESCO-linked international scientific organizations with the declared purpose of promoting 'peacefare.' This was the context in which the International Union of Psychological Science (IUPsyS), an international body of national psychological associations, was established in 1951.

The IUPsyS started with eleven, mainly European, charter members. In 1999 membership reached a total of 68 countries (Rosenzweig, 1999). This growth strikingly reflects the global career of American-style psychology which, rivaled in only a few areas by Soviet psychology, soon superceded British and other colonially-established influences in the Third World. From a celebratory view the international congresses held in Asia (Tokyo 1972), Latin America (Acapulco 1984) and Australia (Sydney 1988) marked "a genuine global expansion of international psychology" (Rosenzweig, Holtzman, Sabourin, & Bélanger, 2000, p. 195). Membership grew rapidly after the Acapulco and Sydney congresses. More importantly, the social constituency of membership changed considerably, including representatives from increasing numbers of Third World countries and from the nations succeeding the collapse of the Soviet Union.

The very existence of the IUPsyS encouraged the formation of national psychological organizations which, in turn, presupposed the shaping of a disciplinary identity. As argued by Kitty Dumont and Johann Louw (2001), membership in the IUPsyS conveyed a sense of participating in a 'universal' discipline and could also be used to legitimize the discipline in a given country. Their case study of the psychological associations in South Africa and the German Democratic example illustrates the difficulties encountered by psychologists in legitimizing their discipline within the conflicting objectives of national politics and the IUPsyS's apolitical constitution. More case studies on the politics of membership would increase our comprehension of the IUPsyS's role in the shaping of the discipline's 'internationalization.'

The IUPsyS strongly emphasized international exchange. According to the 1951 IUPsyS statutes its aims include the development of intellectual exchange and scientific relations between psychologists of different countries, assistance for scholars of different countries to go abroad to universities, and the exchange of students and young research workers. Yet at least in the formative years of the IUPsyS, the emphasis on international exchange reads like a euphemism. In the

post-war and Cold War social and intellectual context the discipline's promotion in the non-Western world is more accurately described as a unidirectional transfer from center to periphery.

As to the intellectual context of this transfer, the conceptual framework of US dominance in 'peacefare' attempts after World War II was made up of behavioral science, the master ideology of developmentalism, and modernization theory. American psychologists, in particular, felt well-prepared to take the lead. As amply documented by Ellen Herman (1996), the World War II generation of American psychologists saw it as their postwar duty to construct a 'behavioral science' that would be theoretically and practically suitable for the prediction and control of human behavior in all areas of social life, at home and abroad. Offering their services to policy-makers they "aimed at nothing less than to 'fashion a new civilization' and 'restructure the culture of the world'" (Herman 1996, p. 306). The "career of cold war psychology" (Herman) worked on the assumption that 'civilization' can be conceptualized and studied in terms of the conditions of individual development rather than of social conditions. Adopted by the sponsorship of the Ford Foundation, this basically ahistoric and socially reductionistic conception of behavioral science was to dominate the post-Second World War period.

The master ideology of developmentalism provided the umbrella for interpreting socio-economic and political processes at home and in 'developing societies.' As analyzed by Ozay Mehmet, the new agenda for economists consisted in the "modeling of economic development as a linear, homogenized process" (1995, p. 56). Based on neoclassical versions of the market theory of capitalism with its key assumption of rational, self-interested economic man, this agenda made the Third World look irrational. In political terms, the new agenda of fostering national development with a kind of Marshall Plan of foreign aid was premised on modernization theory. In this theory, modernization was conceived in dichotomous terms of traditional and modern, Western and non-Western. It entailed "a view of the world where the particular experience of one country, notably the United States, was the yardstick against which the achievements and failures of other countries were measured" (Wittrock, 2001, p. 30). Combined with an individualistic view of society, this evolutionary model of a linear path to modernity also inspired attempts to address the subjective dimension of becoming modern.

In such attempts the category of motivation that had emerged in the inter-war period played a prominent role. Danziger's analysis of the abstraction involved in this category needs to be recalled to grasp the rationale of unifying wants, desires, and motives "insofar as they were potential objects of manipulation and influence" (1997, p. 115). The establishment of a "minor industry devoted to 'achievement motivation'" (p. 122) fit the purposes of foreign aid policy well. Conceiving economic development as a result of achievement-motivated personalities, psychological experts were then able to measure the motivational resources of a given country and offer their advice on U.S. foreign aid (Herman, 1996). Psychologists

in a Harvard-Stanford project on 'Social and Cultural Aspects of Modernization' even designed a model of individual modernity (Inkeles & Smith, 1974). In this case the requirements of factory life informed the concept of 'individual modernity' translated into attitude scales. Thus turned into individual attributes, people's sense of efficacy, their respect for science and technology, their acceptance of time discipline, and the like, were measured and served as a comparison of 'human modernity resources' in Argentina, Chile, India, Israel, Nigeria and East Pakistan.

Like anthropologists in the colonial period, psychologists exploring the motivational and attitudinal resources of non-Western populations facilitated Western science's entry into native cultures. Their investigative practices entailed the recruitment of local experts to assist them with various tasks such as communication with local institutions (e.g., schools) and the collection of data. School teachers and the like thus became recipients and transmitters of a psycho-technological toolbox even before psychology was established at the universities of their countries. For instance, at the time LeVine's (1966) study on achievement motivation in Nigeria was under way there were but a few African scholars taking a post-graduate degree in education at Western universities—degrees which would later entitle them to academic positions at home (Durojaiye, 1993). Closer historical analysis would thus be well advised to explore the networks of local assistants involved in carrying out the projects designed by behavioral scientists and funded on a large scale by the Ford and Carnegie Foundations. The 'acknowledgements' in samples of published studies might provide some clue to such analysis. To address further questions concerning "the patterns of interaction between colony and imperialist power in the areas of scientific and technological development, in terms of the derivation of ideas, exploitation one-way or mutual, emulation or rejection" (Turtle, 1987, p. 3) requires attention to the responses from the receiver side.

DISCONTENT WITH THE IMPORTED PRODUCT

The response of Third World psychologists to the imported discipline must be seen in the context of grave disparities in the structure of international knowledge production and distribution. For a considerable period US-domination in the international knowledge network operated as a one-way flow from 'center' to 'periphery' that largely turned Third World scholars into recipients and was perceived by them in this way. The impact of UNESCO efforts to promote exchange of knowledge for endogenous development remained, at best, slow (Schwendler, 1984). As described by Fatali Moghaddam (1987), when there were still three worlds, the first and second rivaled to influence the third, but the first world was the major producer of psychological and other scientific knowledge and distributed this knowledge to the other two worlds without being itself much influenced by

psychologies elsewhere. Western industrial countries—with their concentration of the sources of knowledge production—have remained largely in control of knowledge distribution to and from 'peripheral' countries, for instance by multinational publishing companies. Only a few countries, like Mexico and India, have themselves become regional centers while remaining peripheral in the international knowledge network. India, for instance, has become "a major producer and distributor of knowledge in its own right. It boasts the world's third largest scientific community. Its university system is one of the world's largest, with more than 3 million students enrolled. India is a major publishing nation, ranking eighth in the world, and it exports books to other Third World countries" (Altbach, 1985, p. 110).

Concern with the unbalanced structure of international knowledge production has thus been a pervasive theme among psychologists in Third World countries. They have identified the unilateral dependence on Western sponsoring agencies, research resources, conference and publication practices as both systemic deterrents to discipline development and a major reason for the lack of cooperation among Third World countries. In some cases, like Iran, unilateral dependence of local universities on particular US universities has even had the effect that institutional developments within the country remained uncoordinated (Moghaddam, 1993).

The response to the imported product itself has varied from unquestioned cloning to attempts at identifying the faults of the product often along with a 'call for indigenization.' Historical accounts from particular countries are still rare, but most reports of Third World psychologists available in Western publications provide enough historical context to permit a sketch of contexts and changes. In the late 1960s and early 1970s post-Independence liberation sentiments joined with the radical Western debate on science, society, and imperialism in the rejection of both the use of imperial language in education and the 'psychologization' of social problems. In the Philippines, for instance, "the teaching of the national language became an important alternative to English" (Lagmay, 1984, p. 34) and a subsequent movement for an indigenous social science (see below). From sub-Saharan Africa and Latin America to Iran, it was stressed that the problems of Third World countries are "to a large extent an historical consequence of their colonization and exploitation by the industrialized nations of Europe and North America" and that their attribution "to psychological factors within the individual members of these societies" would amount to an unethical abuse of psychological concepts to cover up politico-economic realities" (Mehryar, 1984, p. 165). In a response to Gustav Jahoda's (1973) question of whether developing countries need psychology the Ethiopian psychologist Yusuf Omer Abdi strikes a similar key: "The concept of psychology, its theories and methods as understood by Westerners are alien to the thinking of an African ... Africa is involved in an endless war against its arch enemies, namely: hunger and mass starvation, ignorance, disease and economic stagnation"

(1975, p. 230). Rather than a need for the Western product he sees "a need for African-oriented psychology which is based on the needs and problems of the people in Africa" (p. 230). This touches the question of what a socially relevant psychology might be like, a question that was raised in most countries sooner or later. Recent attempts to elaborate on this question show an increased awareness of contextual complexity. For example, the Cameroon psychologist Bame Nsamenang stresses that the development of a socially relevant psychology requires the recognition of both the enormous diversity of sub-Saharan countries and their similarities in terms of "patterns of ecological adaptations" and "common socio-historical experiences" (1995, p. 730).

In India discontent with colonially-established psychology dates back to Independence in 1947. Durghanand Sinha, who in 1961 started the psychology department at Allahabad University and later joined the IUPsyS executive committee, has distinguished three phases of response: (a) an early post-Independence wave of reaction against the mechanistic Western view of humans which sought an alternative in revivalism, (b) a first phase of indigenization of psychology in the 1960s that tried to redirect psychology to the problems of social change and national development, and (c) indigenization proper in the sense of exploring cultural traditions for concepts and models relevant for understanding social reality (cf. Sinha, 1993, pp. 33–36). His own research career mirrors these developments. A prominent argument in the 1960s was that in order to be relevant to a predominantly rural nation, the urban middle-class bias of Western psychology had to be abandoned in favor of a "rural psychology" focused, for instance, on farmers' motivational patterns. He later argued that Western individuo-centric micro-psychology needs to be replaced by a macro-psychology oriented to major social problems and receptive to knowledge developed in other social sciences.

Though Sinha's 'phases of development' have not passed without controversy (cf. Joshi, 1992), there is agreement among many Indian observers that the majority of psychologists in India have been insensitive to the rapid and uneven national socioeconomic transformations. The social role and responsibility of psychologists has been a major concern in this respect. Envisioning a social role for psychologists, Sinha (1984) has emphasized the task of investigating how the subjective dimension of desirable change could be addressed to minimize its alienating and disabling effects. Girishwar Misra strikes a different key in suggesting that the social role of the academic psychologist be envisioned "in terms of understanding, reading, and interpreting cultural actions; sensitizing people to the potentialities of action in the existing range of intelligibilities; and inviting exploration in alternative forms of understanding" (Gergen, Gulerce, Lock, & Misra, 1996, p. 498).

For Sinha, the limited impact of psychology on the pressing problems in the Third World has reasons in "basic limitations of the discipline as it has developed in the West. Though it deals, in principle, with both the social system and individual processes, its orientation is basically microsocial in focusing on personal

characteristics of the individual actors in social processes rather than on socio-structural factors" (1984, p. 23). He also reflects on the historical reasons for this orientation. Addressing the socio-cultural milieu that supported the rise of psychology and related disciplines in Western industrial societies, he emphasizes literacy, secularism, and individualism. In a situation of relative stability these major preconditions, he argues, eased the emergence of a 'micropsychology' geared to increasing individual efficacy.

A brief examination of the status of psychology in Pakistan and Bangladesh after the partition of the Indian sub-continent should help illustrate the legacy of colonialism. As analyzed by Arjun Appadurai (1993), the partition had deep roots in the colonial bio-politics of enumerating and classifying populations that constructed separate Hindu and Muslim identities and imagined communities. It resulted in large-scale migrations on both sides and continuing, volatile conflicts. At institutions of higher learning in post-Independence Pakistan the migrations created a long-lasting void "because there were more Hindus and Sikhs who held these positions than Muslims available to replace them" (Moghni, 1987, p. 26). Moghni reports that in the post-Second World War years American psychology also dominated in Pakistan, with American trained teachers, textbooks, and training programs filtering into educational institutions. After the establishment of more universities and psychology departments a national association of psychologists was formed in the mid-1960s, but it was not before its second meeting in the mid-1970s that the presidential address advised psychologists "to climb the walls of their neat and tidy disciplines and acquaint themselves at first hand with the poor, illiterate and diseased people of their country" (Moghni, 1987, p. 31). The author also observed an initial impact of Sufi doctrine and practice on psychology and a "powerful movement of Islamizing knowledge including psychology" (p. 35) that makes one wonder whether this might explain the admission of Pakistan's national association of psychologists to IUPsyS membership in 1987, nearly 20 years later than their Indian counterpart (Rosenzweig et al., 2000, Appendix E).

As to Bangladesh, Begum (1987) has observed a lasting influence of American psychology in applied areas like personnel selection for the army and industry through the pre-independence period. After Independence in 1971, English was replaced by Bengali (Bangla) in education and psychologists began to write text-books in the national language. A national association of psychologists was formed and a nation-wide journal established, alongside the existing Dhaka University journal. By the early 1980s three professional organizations each addressed issues in clinical psychology and the care and education of mentally disabled people. A likely reason for this professional focus may be found in the 'Health for All 2000' report which mentions disabilities due to malnutrition, especially blindness and retardation, among the most pressing problems of the country. Much applied research consisted of adapting Western tests for use in Bangladesh, yet for the period covered by his report Begum observed "an increasing awareness amongst

local psychologists of the shortcomings of their work" (1987, p. 69). In 1996, the national Bangladesh association of psychologists became a member of the IUPsyS (Rosenzweig et al., 2000, Appendix E). Given that for more than a decade after the partition there were less than five universities in Pakistan, millennium listings indicate an explosive growth with 38 universities in Pakistan and 30 in Bangladesh, as compared to 96 in India (Förster, 1999–2001).

The polyphonic objections against Western psychology certainly stress Danziger's observation that the more psychologists in Asia, Africa and Latin America "are raising questions about their own traditions and their relationship to the theory and practice of psychology . . . the more dissatisfied they become with the parochialism of a historiography of psychology anchored in North American and European perspectives" (Danziger, 1994, p. 477).

THEORETICAL ATTEMPTS TO INDIGENIZE PSYCHOLOGY

As discussed in 1979 by Krishna Kumar, the term 'indigenization' may refer to three different levels of directing social science toward the particular situation and problems of a nation (in Turtle, 1987, p. 15). The 'structural level' concerns the institutional and organizational resources for the production and diffusion of social science knowledge in a given country. The 'substantive level' entails the premise that social science ought to focus on nation-relevant issues. While these two meanings of 'indigenization' are widely accepted among Third World psychologists, the 'theoretical level,' addressing the search for alternatives to Western conceptual frameworks, generated a great deal of debate.

Approaches to theoretical indigenization are geared to the development of a distinctive conceptual framework able to reflect the culturally-rooted understanding people have of themselves and of the world. Considering the 'peculiarity' (Geertz) of the notion of the individual person from the perspective of other cultures, the rationale for theoretical indigenization is obvious. It involves a recognition of the relationship between collective histories in particular socio-cultural life-worlds and particular ways of mapping the human condition, which may be combined with a self-conscious assertion of 'otherness.' The assertion of otherness shows in trends of a 'sinification' (Hsu, 1987), 'Islamization' (Moghni, 1987) or 'Africanization' (Myers, 1993; Holdstock, 2000) of psychology. For example, 'Afrocentric' views of psychology and social science as currently advanced in African-American studies and some African quarters define 'Afrocentricity' as a focus on generating knowledge grounded in the life experience of Africans or people of African descent and geared to empower them to improve their collective life conditions (Hamlet, 1992). The historical roots of the conceptual frameworks used to characterize 'Afrocentric' views date back to the pre-independence generation of African intellectuals like Aimée Césaire, Léopold Senghor and Alioune Diop.

Challenging the ideology of white supremacy, they advanced a self-assertive view of African self-being (*'négritude'*) and participation in a natural, social and spiritual harmony. Mudimbe (1988) provides a sophisticated discussion of the problems involved in *'négritude.'* As outlined by the African-American psychologist Linda James Myers (1993), the framework for an 'optimal psychology' entails an all-encompassing opposition between a 'Eurocentric' and an 'Afrocentric' worldview and philosophy of science. From a cosmic spiritual ontology, a holistic epistemology, and an axiology that values positive human relations highest, she derives psychological concepts like lived holism, the spiritual self, and the other-centered person. In a similar vein, Len Holdstock (2000) discusses holism as a lived experience in Africa with regard to the cognitive, interpersonal, intrapersonal, and aesthetic dimension (Ch. 10).

Though such contributions accentuate the difficulties of reorienting the views and priorities of people of African descent after a long history of colonization, the alternate Afrocentric views of human being and action are problematic essentializing constructions. The way in which Myers and Holdstock depict the spirituality, sense of unity and other-centeredness as features of 'Africanity' suggests a reading that takes these features as timeless characteristics of human nature in Africa. I agree with Johann Louw in that they "come close to a position that makes these categories of human nature appear self-evident, 'natural,' and trans-historical" (2002, p. 9).

A different approach is demonstrated by attempts to 'decoloniz[e] the Filipino psyche' by developing a Filipino psychology (Enriquez, 1987; 1993). Opposed to the Marcos regime, the National Association for Filipino Psychology, established in 1975 by Virgilio Enriquez (Lagmay, 1984), advocated the use of Filipino language and the development of Filipino identity and national consciousness. It aimed at developing "all aspects of the Filipino consciousness towards an active scientific and Universal psychology" (Enriquez, 1987, p. 283). Open to social scientists, intellectuals and artists committed to a Filipino perspective, the association held annual conferences, published their proceedings, and initiated special courses at the University of the Philippines. A community field station in a rural area was established; in the mid-1980s a Philippine Psychology Research and Training House headed by Enriquez expanded into an Academy. In 1982 his achievements earned Enriquez a distinguished award as Outstanding Young Scientist in Social Science by the National Academy of Science and Technology (Lagmay, 1984). Filipino psychology is field-oriented, geared to turning "regionalism and language diversity in the Philippines into an advantage" for enriching national culture (Enriquez, 1987, p. 281). Envisioning the prospects for the 1990s, Enriquez stresses the conduct of further "studies on Filipino behavior and psychology, Filipino personality, Philippine language, culture and history by using appropriate and culturally relevant theory and methodology" (p. 285). To this end the Academy "includes historians, artists, and scientists in its staff based on the avowed belief that the Filipino

psyche is too important to leave in the hands of psychologists alone" (p. 285). The search for core Filipino concepts and their implied values is not designed as an end in itself, but as a step in the 'cross-indigenous' method geared toward a global psychology (Enriquez, 1993).

A rich spectrum of approaches to theoretical indigenization is documented in India. Among the early advocates of a theoretical grounding of psychology in the classic traditions was Ashis Nandy, who pleaded in 1974 for an 'alternative culture of psychology in India,' calling upon Indian psychologists "to participate actively in a reconstruction of the philosophical basis of psychology" in order "to generate new culture-sensitive theories" (quoted in Turtle, 1987, p. 16). Twenty years later Girishwar Misra outlined an Indian perspective on reality and human functioning on the basis of scholarly reconstructions of philosophical-psychological perspectives on Indian culture. He emphasized a holistic worldview, a relational concept of the person as well as contextualized relationships and a Dharma (duty)-centered moral code (Misra & Gergen, 1993, p. 233).

As to the impact of attempts at theoretical indigenization on psychology as a discipline, one of Sinha's more recent comments was that after twenty years many psychologists, even in India, "are finding it difficult to cast off the microcosmic and individualistic orientation acquired in the West" as they are bound by its prevailing disciplinary ethos (1993, p. 40). A different perception underlies Girishwar Misra's statement that "the universally projected modernist view of the individual as a self-determining and self-contained being is rapidly losing its functional value" among many Indian psychologists (Gergen, Gulerce, Lock, & Misra, 1996, p. 489).

A somewhat more comprehensive picture of the impact of theoretical indigenization on psychology in India can be gained from the state-of-the-art surveys initiated and supported by the Indian Council of Social Science Research and published every ten years (Mitra, 1972; Pareek, 1980; Pandey, 1988; 2001). To date two of the three volumes of the most recent survey have been published and tempt me to offer some observations. The editor's preface to the series advances the view that research on 'Physiological foundation and human cognition' (vol. I) still depends strongly on Western psychology, whereas surveys on 'Applied social and organizational psychology' (vol. III) contain evaluations "from a theoretical and cultural perspective" (Pandey, 2001, p. 11). The second volume, on 'Personality and Health Psychology,' includes areas not covered in previous surveys: consciousness studies, gender issues, and health psychology. The review of consciousness studies reflects on Indian and Western theoretical perspectives, suggesting that despite "the clear advantage that Indian tradition bestows on the area," notably in the transcendental realm, "Indian psychologists have in general been preoccupied with more mundane issues" (Rao, 2001, p. 138). For the reviewer of feminist perspectives in psychology, they have regrettably "remained on the periphery of mainstream Indian psychology" (Bharat, 2001, p. 308). Answering the hypothetical question of whether a psychology of the Indian women is evolving, she arrives at a 'no'

based on observations of both "weak conceptualization" and "a lack of continuity in contemporary research efforts to span the spectrum of issues related to women's being and existence," due to "academic isolation" from interdisciplinary women's studies (Bharat, 2001, p. 346, 347, 348). In his chapter on 'Personality, Self and Life Events,' Naidu echoes the reviewers of the three previous surveys when he writes that "most of the studies are atheoretical, imitative of western trends and of indifferent quality;" in this respect he notes only a slight increase of quality and professionalism through the 1990s (2001, vol. 2, p. 230). A declared favorite of an indigenous perspective, he calls attention to some recent studies on Vedantic views of the spiritual nature of the self. Yet as to the question of progress in indigenizing psychology in India, he can but refer to the sobering results of the empirical assessment conducted by Adair, Puhan, and Vohra (1993).

Western and non-Western psychologists advocating a place for culture in psychology have put their hope in intercultural dialogue, envisioning a mutual offering of manifold notions of person, experience, and social relations. There is, however, some romanticism in this sort of praise for the variety of cultural concepts and practices in as much as it fails to reflect the power structure of the discipline. Aydan Gulerce's 'Turkish Vision' evidences an awareness of the deeper conditions for a genuine intercultural exchange when she states that it can only be hoped for once "the West has gained sufficient self-reflexivity to prevent further patronizing and the rest of the world has gained sufficient self-assertion for emancipation" (Gergen, Gulerce, Lock, & Misra, 1996, p. 501). Explicating these conditions, I argue that psychology is but one component of a disciplinary apparatus which needs to be reflected upon in terms of a particular cultural construction of reality that emerged in Europe along with colonialism.

TRANSCENDING DISCIPLINARY BLINDFOLDS

Critics of the cloning of Western psychology have shown an awareness of both the methodological individualism implied in the Western model and its particular cultural roots. There is, however, a striking absence in both the articulations of discontent with Western psychology and the attempts to indigenize. What escapes reflection is the very disciplinary model that guides "the way we think, perceive and seek to understand reality and the universe in the modern world" (Giri, 1998, p. 380). In the modern disciplinary construction of reality, 'the psychological' is set entirely apart from 'the social,' 'the political' etc., not to mention the 'moral.' The historical roots of this construction date back to modern state formation and the industrial revolutions based on the principle of a scientific division of labour. Through the nineteenth century, Enlightenment discourse on the human historical and social condition has branched into increasingly specialized areas of knowledge production. New subject matters for systematic observation were created along

with new practices of managing social relations, designed in institutional settings such as schools, factories, prisons, and asylums. The formation of social and human science disciplines largely followed the lines of this institutionalized exercise of social and human engineering. Once established, however, the disciplines tend to be taken as simply reflecting 'given' segments of reality.

The intellectual constraints imposed by the transplantation of this disciplinary model to different socio-cultural worlds become obvious, for instance, in pleas for a 'macropsychology' that would—somehow—integrate psychological concepts with those of other social sciences. Yet had psychologists been receptive to other social sciences, the problem would have merely multiplied. Economics, political science, and sociology are no less cultural constructions than psychology. In India and elsewhere, imported economics with its neoclassical version of market theory proved unsuitable for conceptualizing endogenous economic development. Critics like Ozay Mehmet have since pleaded for "a more inductive theorizing . . . grounded in actual social reality that is articulated bottom-up" (1995, p. 148). As Danziger observed, the metalanguage of variables which was subsequently exported along with the technology of quantitative social research had also been adopted within sociology. Yet he also points out that there was "quite a powerful alternative tradition—particularly in the form of symbolic interactionism" (1997, p. 171). Had this invitation to the analysis of subjective meanings ever reached the Third World it might have encouraged attempts at bottom-up social analysis. Another alternative tradition, the reading and reinterpreting of classical social theories from Marx to Max Weber, had, in fact, reached some non-Western quarters. If nothing more, it has allowed for a clearer notion of the kind of capitalist dynamics that account for Third World dependency.

Attempts to transcend the Western disciplinary order have gained some prominence in interdisciplinary centers like the Center for the Study of Developing Societies at Delhi, an independent research institution founded in 1964, and the Madras Institute of Development Studies. Ananta Giri who works at the latter has argued that "transcending disciplinary boundaries" to create new knowledge presupposes an understanding of "the limitations of the discourse and institutions of modernity" that involves "critiques of modernity" itself (1998, p. 379). Drawing on the critique of instrumental reason, she points out that the disciplinary construction of knowledge is based on an instrumental concept of knowledge and its goal of control and domination. She contrasts this concept with knowledge traditions in Asian countries to open the question of alternative modes of knowing. Psychologists, it seems, must personally 'transcend' the boundaries of their discipline. This is illustrated by the example of Ashis Nandy, an early radical critic of psychology. His investigations into political cultures in India have taken him to 'the edge of psychology' (1990) and eventually to the Delhi Center for the Study of Developing Societies. He has since published widely on the critique of developmentalism, science, and hegemony (Nandy, 1994) and has, as Director of the Center, been

involved with the recently-implemented *Forum for Alternative Thinking in South Asia*.

The question remains to what extent creative work conducted at such Centers can be expected to impact the discipline-fixated academy in the non-Western and the Western world. Pleas for alternative thinking have been heard at least in some Western quarters, and there have been journals and occasional conferences that pride themselves of the participation of Third World scholars, especially from centers of excellence. As to Aydan Gulerce's expectation of 'sufficient self-reflexivity to avoid patronizing,' much is still being found wanting. Even well-known intellectuals who seem to move easily between the First and Third World and are articulate in voicing their concerns about the postcolonial predicament, face conditions strongly expressed by Gayatri Spivak:

> "For me, the question 'Who should speak?' is less crucial than 'Who will listen?'
> 'I will speak for myself as a Third World person' is an important position for political
> mobilization today. But the real demand is that, when I speak from that position,
> I should be listened to seriously, not with that kind of benevolent imperialism..."
> (quoted in Smith, 1999, p. 71)

DE-CENTERING WESTERN PERSPECTIVES

For Western and non-Western scholars alike, the positional superiority of the Western construction of reality remains a problem. Incarnated in the category of 'modernity' which, after the collapse of colonialism, was "convenient at once to ex-masters and ex-subjects anxious to restate their inequalities in a hopeful idiom," this construction has become "pervasive, as either a presence or a lack, an achievement or a failure, a liberation or a burden" (Geertz, 1995, pp. 137–138). The categories of 'modernity' and 'modernization' have set the terms in which countries not shaped by capitalism, industrialism, and science "are these days perceived, discussed, analyzed, and judged, both by the world at large and by their own populations" (Geertz, 1995, p. 140). Pondering the changing Moroccan and Indonesian sites of his previous research, Clifford Geertz has titled the chapter 'modernities' in the plural, indicating that there is no discernible common project in the tangle of hopes for the future, rejections of the past, and regrets of what might pass with it. The postmodern idiom of modernities, identities, emancipations, and the like, conveys at least a sense of imaginable alternatives to Western perspectives. Still, in the aftermath of colonial domination and disqualification of ways of life and modes of knowledge, of attempts to reinvent disrupted lives, the scope of alternatives remains defined by previous transformations. What needs to be reflected upon is the "irrevocable process of transmutations" brought about by European imperial dominance, which cannot be done away with as but a "a temporary repression of subject populations" (Asad, 1991, p. 314). This involves a

reflection on the categories and bodies of knowledge formed by the epistemological order of colonialism. "Decolonizing the mind," to use the famous phrase coined by Ngugi wa Thiong'o (1986), is essential for the minds of both the colonized and the colonizers. On neither side can the colonial legacy simply be discarded. What is, however, essential is a reworking, repositioning and restructuring of the received construction of reality.

From the days of Frantz Fanon and Albert Memmi colonized intellectuals and their postcolonial heirs have been 'talking back' and 'writing back' reminding the West of the dislocations brought about by colonialism for both the colonized and the colonizers. They have drawn attention to the processes of colonizing lives and minds, foremost to the ways in which the knowledge gained through colonization has been represented back to the colonized and used to structure their own ways of knowing. Drawing on both the anti-colonial and the Western critical tradition they have analyzed how the basic categories and assumptions of our received knowledge of the world have been shaped by the colonial condition. Their polyphonic challenge to Euro-American modes of thinking history and knowledge has inspired some critical scholarship in anthropology, history and cultural studies, yet it has hardly entered the hard-core social science disciplines, not to mention psychology.

Orientalism (Said, 1978) and colonial discourse at large was about categorizing the world in terms of metropolitan centers and colonial peripheries, about marking sites and people as 'foreign' and 'other,' about managing and 'civilizing' people. It took empirical shape in measuring and classifying land as well as in counting and categorizing populations, exoticizing and essentializing them. Implanted into local politics of difference, as in the case of the 'imagined communities' of Hindu and Moslem nationalism (Appadurai, 1993), the colonial order of knowledge has outlived the end of colonial rule. It is still difficult, for the heirs of the colonizers and the colonized alike, to think "outside of orientalist habits and categories" (Breckenridge & Van der Veer, 1993, p. 11) or even to make out "whether one speaks from within, . . . outside of . . . or at all without" colonial discourses (Nakata, 1995, p. 8).

For postcolonial intellectuals who position themselves within the academy in the Third World, their indigenous communities, and toward the Western world, questions of knowledge and power have been of vital importance. Linda Tuhiwai Smith, Director of the International Research Institute for Maori and Indigenous Studies at the University of Auckland, has probed deeply into Western modes of organizing knowledge, of knowledge production, its producers, and its beneficiaries. In her book on *Decolonizing Methodologies* (1999) she starts with the observation that in the collective memory of the colonized, the notions of discovery, research, and knowledge are closely related to a sense of having been disowned of their knowledge and imagery. Like other indigenous peoples the Maori have long been objects of research, their cultural knowledges and systems of living have become classified, represented to Western audiences and, through the eyes of the West,

represented back to them. Supported by academic and political institutions and organizations, colonial discourse established a positional superiority of Western knowledge. Positioning herself in the liberation movement of indigenous peoples, she advocates a complementing of the political agenda (Wilmer, 1993) with an agenda for research toward the goal of social justice. For indigenous cultural politics, she argues, a constant reworking of their notions of colonial conditions and knowledge production has been essential in developing a language of postcolonial critique that not only permits deconstruction and recontextualization of Western scholarship but also designs alternative research practices and policies. 'Decolonizing methodologies,' she stresses, is "about centering our concerns and world views and then coming to know and understand theory and research from our own perspectives and for our own purposes" (1999, p. 39).

In her critique of the intersections of knowledge, research, and imperialism, Smith outlines the Western cultural views of human nature, gender and race, individual and society, and time and space that underpin Western modes of knowledge. The disciplinary organization of academic knowledge, she argues, is particularly intriguing for indigenous intellectuals who reclaim a voice vis-à-vis the positional superiority of Western science: "Most of the 'traditional' disciplines are grounded in cultural world views which are either antagonistic to other belief systems or have no methodology for dealing with other knowledge systems" (Smith, 1999, p. 65). Due to their shared cultural foundations the disciplines are deeply implicated in each other, but also "insulated from each other" and thus "protected" from outside claims, able "to develop independently" and to keep their histories separate (Smith, 1999, p. 67). In this double-bind of disciplinary implication and insulation controversies over what counts as knowledge are thus easily caught up in either the insular protection of a discipline from other views of the significance of 'facts' or in the tangle of "reconnecting and reordering those ways of knowing which were submerged, hidden, or driven underground" (Smith, 1999, p. 69).

Her account of the historical intersections of imperialism, knowledge, and research provides a strong counterweight to Eurocentric accounts of the rewards of the pursuit of knowledge. Based on the traditions of both postcolonial critique and Western critical theories, she advances a view of research cultures as institutionalized intellectual activities that are closely connected to hegemonic or counter-hegemonic discourses. Thus her book is a convincing argument that a rewriting of the history of modernity and science cannot dispense with the critical views based on the collective experience of the colonized and their heirs.

PROSPECTS FOR A POLYCENTRIC CRITICAL HISTORY

A polycentric approach to the history of the transcultural migration of psychology as advanced by Danziger (1994; 1996) involves questions that exceed the

confines of the discipline. Emphasizing the contribution by historically-minded Third World psychologists, he has drawn attention to questions concerning the links between psychology and both "cultural imperialism" and "the historical project of modernism" (1994, p. 477). In this chapter I have argued that some challenging studies from the perspective of the colonized and their heirs are indispensable for rethinking the intersection of knowledge, imperial power, and resistance. Drawing on both anti-colonial and Western traditions of critical theory they have repositioned decisive issues pertaining to the Western construction of reality. To elaborate on both this construction and its fragmented embodiment in the disciplinary order historians of psychology and the social sciences can draw on a growing field of studies that historicize the domains taken for granted as universal in the self-understanding of Western modernity. For instance, exploring the genealogies of religion, Talal Asad (1993) has demonstrated that the universalized concept of 'religion' is a 'cultural construction' that originated in European modernity. The view of religion implied in this construction makes it "part of what is *inessential* to our common politics, economy, science, and morality" (p. 207). As a new historical object this notion of 'religion' authorizes particular forms of 'history making,' for instance the degrading of Islamic states as immature vis-à-vis Western secularization. In a similar vein, Paul Rabinow's project to 'anthropologize' the West aims at showing "how exotic its constitution of reality has been" (1996, p. x). Exploring the historicity of "the relations of truth and virtue, power and culture" (p. 138), he has begun to reveal the historical peculiarity of assumed 'universal' domains like rationality, epistemology, science, and economics.

From a polycentric view of world history the very idea of one historical project of modernity in Europe may have to be given up. Peter Gran (1996) has strongly argued that substantially diverse forms of hegemony have emerged from the capitalist penetration of various parts of Europe. He distinguishes four basic forms of hegemony, among which the classic model of 'British bourgeois democracy' is but one. Examining, for instance, the correspondence of the Italian model with hegemony in India and Latin America, he aims to undermine the dominant historiographical paradigm of 'Europe and the rest.' His analysis of the organization of elite and popular culture, including chapters on the mode and social role of historiography, endorses the view "that people in different hegemonies are not likely to share the same 'common sense'" (p. 348). Gran's approach thus might offer a frame for renewed attempts at exploring the historicity of subjectivities in various parts of the world.

Certainly, a polycentric approach to the historicity of disciplines, hegemonies, subjectivities, and modernities can draw on rich resources of historical sophistication. The emphasis on 'poly-centricity' entails a notion of lived experience, views and voices from various centers as well as a notion of interrelations among centers. Thus the realization of a polycentric critical historiography will essentially depend

on the participation of scholars from Third World, indigenous, and diasporic academic spheres.

ACKNOWLEDGEMENTS

REFERENCES

Abdi, Y. O. (1975). The problems and prospects of psychology in Africa. *International Journal of Psychology, 10*(3), 227–234.

Adair, J. G., Puhan, B. N., & Vohra, N. (1993). Indigenization of psychology: Empirical assessment of progress in Indian research. *International Journal of Psychology, 28*(2), 149–169.

Altbach, P. G. (1985). Centre and periphery in knowledge distribution: an Asian case-study. *International Social Science Journal, 37*(103), 109–118.

Appadurai, A. (1993). Number in the colonial imagination. In C. A. Breckenridge & P. van der Veer (Eds.), *Orientalism and the postcolonial predicament. Perspectives on South Asia* (pp. 314–340). Philadelphia: University of Pennsylvania Press.

Asad, T. (1991). Afterword: From the history of colonial anthropology to the anthropology of Western hegemony. In G. W. Stocking (Ed.), Colonial situations (pp. 314–324). Madison: The University of Wisconsin Press.

Asad, T. (1993). *Genealogies of religion: Discipline and reasons of power in Christianity and Islam*. Baltimore: The Johns Hopkins University Press.

Bharat, S. (2001). On the periphery: The psychology of gender. In J. Pandey (Ed.), *Psychology in India revisited—developments in the discipline*, Vol. 2 (pp. 300–355). New Delhi: Sage.

Begum, H. A. (1987). Psychology in Bangladesh. In G. H. Blowers & A. Turtle (Eds.) *Psychology moving East: The status of Western psychology in Asia and Oceania* (pp. 65–70). Boulder: Westview Press.

Breckenridge, C. A. & van der Veer, P. (Eds.). (1993). *Orientalism and the postcolonial predicament. Perspectives on South Asia*. Philadelphia: University of Pennsylvania Press.

Brock, A. (1994). *An interview with Kurt Danziger*. http://psychology.dur.ac.uk/eshhs/newsletter/interview.htm. Retrieved February 2002.

Danziger, K. (1979). The social origins of modern psychology. In A. R. Buss (Ed.), *Psychology in social context* (pp. 27–45). New York: Irvington.

Danziger, K. (1990). *Constructing the subject: Historical origins of psychological research*. Cambridge: Cambridge University Press.

Danziger, K. (1994). Does the History of Psychology have a future? *Theory & Psychology, 4*(4), 467–484.

Danziger, K. (1996, August). *Towards a polycentric history of psychology*. Paper presented at the XXVI International Congress of Psychology. Montreal, Canada.

Danziger, K. (1997). *Naming the mind: How psychology found its language*. London: Sage.

Dirks, N. B. (Ed.). (1992). *Colonialism and culture*. Ann Arbor: The University of Michigan Press.

Dumont, K. & Louw, J. (2001). The International Union of Psychological Science and the politics of membership: Psychological Associations in South Africa and the German Democratic Republic. *History of Psychology*, 4(4), 388–404.

Durojaiye, M. O. A. (1993). Indigenous psychology in africa: The search for meaning. In U. Kim & J. W. Berry (Eds.), *Indigenous psychologies: Research and experience in cultural context* (pp. 211–220). London: Sage.

Enriquez, V. G. (1987). Decolonizing the Filipino psyche: Impetus for the development of psychology in the philippines. In G. H. Blowers & A. M. Turtle (Eds.), *Psychology moving East: The status of Western psychology in Asia and Oceania* (pp. 265–287). Boulder: Westview Press.

Enriquez, V. G. (1993). Developing a Filipino psychology. In . Kim & J. W. Berry (Eds.), *Indigenous psychologies: Research and experience in cultural context* (pp. 152–169). London: Sage.

Förster, K. (1999–2001). Universities Worldwide. geowww.uibk.ac.at/univ/world.html. Retrieved December 2001.

Geertz, C. (1983). *Local knowledge: Further essays in interpretive anthropology*. London: Fontana Press.

Geertz, C. (1995). *After the fact: Two countries, four decades, one anthropologist*. Cambridge: Harvard University Press.

Gergen, K. J., Gulerce, A., Lock, A. & Misra, G. (1996). Psychological science in cultural context. *American Psychologist, 51*(5), 496–503.

Giri, A. K. (1998). Transcending disciplinary boundaries. *Critique of Anthropology, 18*(4), 379–404.

Gould, S. J. (1981). *The mismeasure of Man*. New York: W. W. Norton & Company.

Gran, P. (1996). *Beyond Eurocentrism: A new view of modern world history*. New York: Syracuse University Press.

Hamlet, J. D. (Ed.). (1992). *Afrocentric visions: studies in culture and communication*. Thousand Oaks, CA: Sage.

Herman, E. (1996). *The romance of American psychology: Political culture in the age of experts*. Berkeley: University of California Press.

Holdstock, T. L. (2000). *Re-examining psychology. Critical perspectives and African insights*. London: Routledge.

Hsu, J. S. Z. (1987). The history of psychology in Taiwan. In G. H. Blowers & A. Turtle (Eds.), *Psychology moving East: The status of Western psychology in Asia and Oceania* (pp. 127–138). Boulder: Westview Press.

Inkeles, A. & Smith, D. H. (1974). *Becoming modern: Individual change in six developing countries*. Cambridge: Harvard University Press.

Jahoda, G. (1973). Psychology and the developing countries: do they need each other? *International Social Science Journal, xxv*(4), 461–479.

Jahoda, G. (1999). *Images of savages: Ancient roots of modern prejudice in western culture*. London: Routledge.

Joshi, M. C. (1992). India. In V. S. Sexton & J. D. Hogan (Eds.), *International psychology: Views from around the world* (pp. 206–219). Lincoln: University of Nebraska Press.

Kim, U. & Berry, J. W. (Eds.). (1993). *Indigenous psychologies: Research and experience in cultural context*. London: Sage.

Lagmay, A. V. (1984). Western psychology in the Philippines: Impact and response. *International Journal of Psychology, 19*, 31–44.

LeVine, R. A. (1966). *Dreams and deeds: Achievement motivation in Nigeria*. Chicago: The University of Chicago Press.

Louw, J. (2002). Psychology, history, and society. *South African Journal of Psychology, 32*, 1–8.

Mehmet, O. (1995). *Westernizing the Third World: The Eurocentricity of economic development theories*. London: Routledge.

Mehryar, A. H. (1984). The role of psychology in national development: Wishful thinking and reality. *International Journal of Psychology, 19*, 159–167.

Misra, G. & Gergen, K. J. (1993). On the place of culture in psychological science. *International Journal of Psychology, 28*(2), 225–243.

Mitra, S. K. (Ed.). (1972). *A survey of research in psychology*. Bombay: Popular Prakashan.

Moghaddam, F. M. (1987). Psychology in the Three Worlds as reflected by the crisis in social psychology and the move toward indigenous third-world psychology. *American Psychologist, 42*, 912–920.

Moghaddam, F. M. (1993). Traditional and modern psychologies in competing cultural systems: Lessons from Iran 1978–1981. In U. Kim & J. W. Berry (Eds.), *Indigenous psychologies: Research and experience in cultural context* (pp. 118–132). London: Sage.

Moghni, S. M. (1987). Development of modern psychology in Pakistan. In G. H. Blowers & A. Turtle (Eds.), *Psychology moving East: The status of Western psychology in Asia and Oceania* (pp. 23–38). Boulder: Westview Press.

Mudimbe, V. Y. (1988). *The invention of Africa: Gnosis, philosophy, and the order of knowledge*. Bloomington: Indiana University Press.

Myers, L. J. (1988). Understanding an Afrocentric Worldview: Introduction to an Optimal Psychology. Dubuque, IO: Kendall/ Hunt.

Naidu, R. K. (2001). Personality, self and life events. In J. Pandey (Ed.), *Psychology in India revisited—developments in the discipline*, Vol. 2 (pp. 228–299). New Delhi: Sage.

Nakata, M. N. (1995). Keynote adress—culture in education: A political strategy for us or for them? In O. Nekitel, S. Winduo & S. Kamene (Eds.), *Critical and developmental literacy* (pp. 8–15). Port Moresby: University of Papua New Guinea Press.

Nandy, A. (1990). *At the edge of psychology: Essays in politics and culture*. Delhi: Oxford University Press.

Nandy, A. (1994). Culture, voice and development: A Primer for the unsuspecting. *Thesis Eleven, 39*, 1–18.

Ngugi wa Thiong'o, J. (1986). *Decolonizing the mind: The politics of language in African literature*. London: James Currey.

Nsamenang, A. B. (1995). Factors influencing the development of psychology in Sub-Saharan Africa. *International Journal of Psychology, 30*(6), 729–739.

Pandey, J. (Ed.). (1988). *Psychology in India: the state-of-the-art*, (3 vols.). New Delhi: Sage.

Pandey, J. (Ed.). (2001). *Psychology in India revisited—developments in the discipline* (3 vols.). New Delhi: Sage.

Pareek, U. (Ed.) (1980). A survey of research in psychology, 1971–1976 (Part I). Bombay: Popular Prakashan.

Polanyi, K. (1944). *The great transformation*. New York: Rinehart and Co.

Probst, P. (1992). Angewandte Ethnologie während der Epoche des Deutschen Kolonialismus (1884–1918). *Psychologie und Geschichte, 3*(3/4), 67–80.

Rabinow, P. (1996). *Essays on the anthropology of reason*. Princeton, N. J.: Princeton University Press.

Rao, K. R. (2001). Consciousness studies: A survey of perspectives and research. In J. Pandey (Ed.), *Psychology in India Revisited—Developments in the Discipline*, Vol. 2 (pp. 19–162). New Delhi: Sage.

Richards, G. (1997). *'Race', racism and psychology: Towards a reflexive history*. London: Routledge.

Rosenzweig, M. R. (1999). Continuity and change in the development of psychology around the world. *American Psychologist, 54*(4), 252–259.

Rosenzweig, M. R., Holtzman, W. H., Sabourin, M. & Bélanger, D. (2000). *History of the International Union of Psychological Science (IUPsyS)*. Hove, East Sussex: Psychology Press.

Said, E. W. (1978). *Orientalism. Western Conceptions of the Orient*. London: Routledge & Kegan Paul.

Schwendler, W. (1984). UNESCO's project on the exchange of knowledge for endogenous development. *International Journal of Psychology, 19*, 3–15.

Sinha, D. (1984). Psychology in the context of Third World development. *International Journal of Psychology*, 19, 17–29.

Sinha, D. (1993). Indigenization of Psychology in India and Its relevance. In U. Kim & J. W. Berry (Eds.), *Indigenous psychologies: Research and experience in cultural context* (pp. 30–43). London: Sage.

Smith, L. T. (1999). *Decolonizing Methodologies. Research and Indigenous Peoples*. Dunedin: University of Otago Press.

Stocking, G. W., Jr. (Ed.) *Colonial Situations: Essays on the Contextualization of Ethnographic Knowledge*. Madison: The University of Wisconsin Press.

Turtle, A. M. (1987). Introduction: A silk road for psychology. In G. H. Blowers & A. Turtle (Eds.), *Psychology moving east: The status of Western psychology in Asia and Oceania* (pp. 1–22). Boulder: Westview Press.

Wilmer, F. (1993). The indigenous voice in world politics. London: Sage.

Wittrock, B. (2001). Social theory and global history: The three cultural crystallizations. *Thesis Eleven, 65*, 27–50.

CONCLUDING COMMENTS

KURT DANZIGER

AGAINST THE GRAIN

History and psychology do not make easy bedfellows. Where undergraduate students are free to concentrate on two subjects of their choice the combination of history and psychology is rarely encountered. Where institutions limit students' choices this combination often becomes a curricular impossibility. But here pedagogy and popular stereotypes merely reflect the fact that in modern times the disciplines of history and psychology have tended to define themselves against each other. History has been closely identified with narrativity and the rich contextualization of particular events whereas psychology has strenuously sought the status of a natural science producing universalistic generalizations that would apply across all times and all places.

What the contributions to this volume all have in common is their transgression against this rigid boundary. In that sense they all go against the grain of prevailing disciplinary orthodoxies. In different ways they all temporalize, and therefore historicize, the subject of psychology. Historical considerations become critical for gaining an understanding of what modern psychology is all about. This holds for those contributors, Bayer and Walsh-Bowers, whose gaze is primarily turned to the discipline's future as much as for those who are more directly concerned with its past. There is implicit agreement that reflection on the status of the discipline must start with the recognition that psychology and its subject matter are situated in historical time.

Although this shared insight involves an assault on the wall that separates psychology and history, this is by no means an "interdisciplinary" enterprise. There are no historians among the contributors, they are all psychologists. Their focus is on disciplinary history, which, in several cases (especially Bayer, Staeuble,

and van Hoorn), takes the form of disputing its boundaries. With one exception (Stam) these authors are not concerned with history as such, the question is what historical studies can do for psychology.

For the contributors to this book that question has become quite explicit. It was not always so. Early examples of disciplinary history were hardly notable for their historiographic interest or sophistication. However, this did not preclude an implicit commitment to a particular agenda. When psychologists turned to the history of their discipline certain themes always seemed to emerge. One theme that appeared early was the special position of experimental psychology for the development of the discipline. This theme famously informed the field's most successful text, E.G. Boring's (1929) *History of experimental psychology*, though in a less strident form it is already present in Klemm (1914). It was a theme that lived very comfortably with psychology's aspirations as a natural science. It also provided a convenient way of dealing with the tensions that had arisen in the wake of psychology's premature institutionalization as a discipline, when it actually lacked a core and was simply a loose assembly of "schools" and special interest areas. The experimental method, as understood by the psychologists of the time (see Winston, this volume), would supply the missing core of psychology. A history of the discipline which privileged its experimental component would therefore supply a sense of unity that counteracted the centrifugal influence of diverse goals and practices. This proved to be a highly acceptable recipe within the discipline so that Boring's text became the fountainhead for many subsequent, mostly American, textbooks prescribed for several generations of students who would find in them an intimation of disciplinary unity that they would not find in the rest of their psychology curriculum.

A second theme that emerged in earlier disciplinary histories was the claim that the subject matter and the concerns of modern psychology were of ancient origin, a reassuring thought for a parvenu among the sciences. In its most explicit form, exemplified by the work of R.I. Watson (1971), this claim led to what one historian characterized as "ahistorical history" (Ash, 1983). On this view the fundamental problems of psychology had always been the same—problems of 'personality', for example, were already an issue in Homer's time—though we now have better methods of dealing with them. Of course, this approach was not only compatible with the privileging of contemporary methodology, it also converged beautifully with the presuppositions of a discipline that was dedicated to the production of universally valid, ahistorical generalizations about human nature.

These prominent tendencies within the field of psychological disciplinary history presented striking examples of what has long been referred to as *justificationist* history (Agassi, 1963; Young, 1965). This meant providing a historical justification for currently favored presuppositions, biases, practices, conceptualizations, and interests within the discipline. Historical scholarship came a distant second to the primary function of the field which was pedagogical, imparting an appropriate group image to aspirant members of the discipline.

Though the scholarly contributions of the field may have been insignificant the sheer size of its pedagogical enterprise, especially in North America, led to the formation of interest groups that began as little more than hobby groups but grew into a recognizable sub-discipline with its own journals, associations, and scholarly networks. This process was certainly fostered by the inhospitable climate within the discipline for any combination of a historical perspective with more conventional psychological interests and practices. Any tolerance for history was limited to the pedagogical services that it was expected to supply. Those whose historical interests went any further were obliged to seek out the company of their own kind. History of psychology's emergence as a sub-discipline of psychology was also fostered by the widespread lack of interest in the topic among those whose disciplinary affiliation was with history. This was not the kind of topic to which historians had traditionally dedicated themselves.

Perhaps it was inevitable that the formation of even a loosely institutionalized sub-discipline should prove favorable to the development of a certain sense of autonomy among its members, especially as it often attracted individuals who were not quite content to plough the furrows prescribed by conventional disciplinary norms. That sense of autonomy enabled an increasing number of those affiliated with the sub-discipline to reject the traditional servant role of the disciplinary historian and to turn away from the discourse of justificationist history. What was also important was that disciplinary historians were potentially far more exposed to developments in the humanities and the social sciences than their colleagues who were protected from such influences by the rigid boundaries that defined the discipline of psychology in general. At any rate, a different, more critical, kind of disciplinary history did emerge during the last quarter of the 20th century.

The contributions collected in this volume are all representative of this trend. They have in common a historicizing of the subject matter of psychology and an emphasis on the role of culture and society in the formation of both psychology and the objects of its attention. It is perhaps surprising that such an orientation should exist within a discipline notable for its pervasive ahistoricism and aculturalism. But in spite of undeniable pressures for disciplinary isolation from the social sciences and humanities the barriers that psychology has erected around itself are not impermeable. There are weaknesses in the rigidity of these disciplinary boundaries for which the present volume provides some telling examples. Some of these weaknesses are associated with local variations in the cultural geography of the discipline. Thus, as Johann Louw's chapter indicates, the interdisciplinarity of my own tastes was surely assisted by a quasi-colonial environment in which disciplinary ties were relatively weak and the salience of socio-political factors incredibly strong. However, one does not need to be existentially thrown into such a situation, as I was; one can choose to abandon the blinkers of a first world outlook, as Irmingard Staeuble has done.

The relative prominence of Dutch and Canadian contributions to this field (not only in this volume but also more generally) seems to confirm the importance of

marginality for its practitioners. In the American heartland of disciplinary psychology isolationism may be supreme but psychologists in the Netherlands have long had to come to terms with often contradictory influences from the major centers, especially Germany and the US (Dehue, 1995; van Strien, 1988). This was often conducive to a more flexible perspective and a critical sensitivity to fundamental issues. Of course I am not suggesting that all Dutch psychologists were strangers to dogmatism and superficiality, but over the years there does seem to have been more mobility of orientation and openness of viewpoint than in the culturally more parochial centers of psychological inquiry. As for Canada, its marginal position with respect to the US hardly needs emphasizing. Disciplinary integration with American psychology has gone a long way but is far from complete. Openness to European and other influences has never been abandoned, not least because of a deliberate rejection of the culture of the melting pot.

Although the creation of a core disciplinary identity has long been a prominent theme in psychology's modern history there has also been a strong push towards disciplinary colonization of more and more areas of human activity. The constant multiplication of "divisions" or special interest groups within professional institutions like the American Psychological Association, as well as the phenomenal growth in the number of new specialist journals, provide evidence for this process. In many cases new areas have been effectively domesticated in terms of the dominant disciplinary ethos. But in other cases disciplinary colonization has not been altogether successful. One such case is that of community psychology, an area represented by one of the contributors to this volume (Walsh-Bowers). Another area, affecting much larger numbers, is that of feminist psychology (Bayer, this volume). It is to be expected that these and other areas which are marginal from the perspective of the core disciplinary ethos will provide a more hospitable climate in which a socio-historical approach is more likely to gain a foothold (Danziger, 1994).

The contributors to this volume have addressed many issues that are relevant to the future development of the field. However, it is hardly possible to comment on all of these issues in this concluding chapter without lapsing into superficiality. In what follows, I have therefore selected some of the more contentious topics for further discussion, because it is these, rather than the issues on which there is broad agreement, which provide the best opportunity for a clarification of my own position within the field as a whole.

THE PERILS OF HISTORY

A common interest in the project of historicizing the subject matter of psychology does not exclude deep disagreements about what exactly such a project entails. The present volume presents a wide spectrum of views that range from

the traditionalist intellectual history of van Rappard to the sociologically oriented "history of the present" that can be found in the chapters of Staeuble, van Strien and Walsh-Bowers. To my mind, what is most significant about this variety of voices is not the dissonance that surfaces from time to time but the fact that psychologists should be discussing such issues at all. A few years ago the critical mass for launching an international project along these lines would not have existed. With some highly localized and mutually isolated exceptions, the disciplinary boundaries protecting psychology's principled ahistoricism would have been too strong, the interest in historical issues too weak, the knowledge of the relevant extra-disciplinary literature too undeveloped. My own work in this field would not have progressed as it did without the emergence of a potential community of interlocutors among some of my psychologist colleagues.

Differences of approach are of course to be expected among an international group such as the contributors to this volume. The development of a historically and socio-culturally oriented approach to the discipline of psychology did not follow the same course in Europe and North America. In Europe the ahistoricism of the discipline was never as pervasive, and the aversion to history never as much in tune with the broader culture, as in North America. When van Rappard informs us that psychologists *do* read the works of the founders of their discipline I am reminded once again that he and I live in very different disciplinary sub-cultures.

Moreover, within Europe, a historical psychology that rejected the investigation of psychological objects outside of time and of history, was able to maintain a foothold in the discipline (examples are Barbu, 1960; Jüttemann, 1986; Peeters, 1996; Scribner, 1985; and Gergen & Gergen, 1984, as the North American exception). Sometimes this led to a historicizing of psychology's disciplinary project (e.g. Jaeger and Staeuble, 1978). As a result, there has been an element of continuity in the field which is lacking in the relevant Anglo-American literature.

That continuity has some advantages, but it has also entailed a certain traditionalism that limits the potential critical impact of this work on other segments of the discipline. One important example of this is the relative neglect of more recent developments in the sociology and historiography of science, though van Strien's chapter in the present volume shows that this may be changing. Those developments have opened up perspectives on the microstructure of scientific investigation which can mediate between the broad sweep of the older histories and the narrow focus of "normal science". As long as this microstructure remained invisible to both historians and scientific practitioners the work of each of them could easily be perceived as irrelevant to the work of the other.

After the second World War a part of European psychology adopted American models and sometimes imported the corresponding historical narrative along with the practices it justified. But indigenous approaches survived and developed their own counter-narratives. Two of these are represented in the present volume (van Hoorn and van Rappard). They are based on venerable models of cultural

and intellectual history respectively. Those approaches may have their uses when it comes to exploring theoretical structures (van Rappard) or pop psychology (van Hoorn), but they offer no means for coming to grips with core problems of psychology's modern history, especially, its successful constitution as a discipline on the basis of very specific scientific and discursive practices. Hence their emphases provide the precise counterpart to the scientistic focus of Boring's widely adopted historiography. Boring had privileged scientific methods and findings while avoiding in depth analysis of theoretical systems and offering only an empty version of the *Zeitgeist* concept as an apology for the absence of socio-cultural contextualization. In contrast, van Rappard's style of history favors theory and van Hoorn's favors cultural trends of the broadest kind. Scientific experimentation, on the other hand, is either dismissed out of hand or not considered worth the same attention to primary sources that theoretical traditions deserve. This can lead to historical assessments whose evidential basis is less than adequate.

Curiously, these cavalier attitudes to experimental practices have an effect that is analogous to the effect Boring-style historiography achieved by its exaltation of experimentation. Whether these practices are not considered worthy of serious historical attention or whether they are regarded as representing the conquest of reality by rationality, the effect is much the same: they are placed beyond critical historical examination. A disciplinary history pursued in this spirit has little to say to the large and influential group of psychologists and lay persons who believe that psychology's scientific status and its claims to a unique expertise depend on its reliance on experimental and quantitative methods. For this group, these methods represent the core of disciplinary endeavor and often of a disciplinary identity defined, not as "psychologist", but as "experimental psychologist". Historicizing the practices that lie at the heart of this disciplinary reality must surely remain a crucial part of the wider project directed at historicizing the subject matter of psychology.

Wittgenstein (1968, p. 232) famously observed that, in psychology, problem and method pass one another by. In the traditional historiography of psychology, whether in its "American" or its "European" form, it is history and method that pass one another by. In the "American" form this effect was achieved by privileging experimental practices and elevating them above history; in the "European" form the same effect is produced by devaluing experimentation to a historically unimportant position, as though psychology were essentially a theoretical affair or simply a cultural phenomenon.

Different ways of historicizing the subject matter of psychology are clearly linked to different ways of interpreting the relationship of psychologist historians to the discipline that is at once the object of their investigations and their institutional home. For historians affiliated with the discipline of history the issue hardly arises: they simply do the job they were trained to do, expecting to find their primary audience among fellow historians, not among the members of

another discipline. For those working in interdisciplinary institutional contexts the question of intra-disciplinary effects may also appear somewhat redundant. But psychologist historians that retain, and expect to continue, their affiliation with the discipline of psychology can rarely avoid this question. The way they resolve it will show in their manner of historicizing the subject matter of the discipline.

With one exception (van Hoorn), the contributors to this volume appear to be in agreement that the rationale for the work of psychologist historians is to be found in its contribution to psychology. In my view, historical exercises that do not do this should be left to historians. Psychologists' ventures into purely historical issues are apt to be unsatisfactory, both from a psychological and a historical point of view, though they may not lack a certain pop appeal. More appropriately, psychologist historians will rely on the work of professional historians for the broader contextualization of the issues that are of primary concern to them. What sorts of issues are these?

One kind of issue arises out of the existence of disciplinary mythologies that often play an important role in the self-understanding of members of the discipline. Disciplinization is not just a matter of a more or less rational division of labor—it also affects peoples' careers, life chances, and sense of self worth. Identification with the progress of a discipline can supply the missing meaning for work that would otherwise seem trivial. In the modern world, disciplines provide important sources of identity, and, like other sources of identity—nations, religions, ethnic groups, etc.—they do this partly through the medium of myth. Such myths often have a significant historical component, including so-called origin myths (Samelson, 1974) that provide recent disciplinary ideologies with a worthy past. Disciplinary historians will inevitably be drawn into debates around the historical components of disciplinary ideologies, either justifying a received version or providing grounds for questioning it.

As is well known, a particular historical narrative regarding the origins of experimental psychology formed part of the professional self-understanding of American psychologists for most of the 20th century. Although elements of this narrative existed quite early, it was given its canonical form in the work of E.G. Boring and then repeated in numerous textbooks that were required reading for aspiring psychologists. Very briefly, this narrative established a historical pedigree for the particular version of experimental psychology that had achieved ascendancy in America (see Winston, this volume). The figure of Wilhelm Wundt necessarily played a significant role in this account, but it was a figure that anyone who had actually studied his work would have trouble recognizing. Clearly, this presented a challenge for disciplinary history (Blumenthal, 1975, 1977), and my early work in this field was very much taken up with meeting that challenge. The resources of intellectual history were often quite adequate for the purpose.

From that point of view it made sense to pose historical questions in terms of "intellectual traditions", as I did in an early paper that situated Wundt in terms of

"two traditions of psychology" (Danziger, 1980), the Lockean and the Leibnizian, a distinction that would have been familiar to many psychologists at the time because it had been used by Gordon Allport (1955). Strong echoes of the notion of a "Leibnizian tradition" can still be detected in van Rappard's (this volume) notion of an "activity tradition". It is an approach that certainly has its uses. Pedagogically it helps students to see beyond the particularities of this or that theory and to pick out underlying commonalities. As van Rappard emphasizes, it also provides a medium that enables the "insider" historians to communicate with their disciplinary colleagues in a non-subversive way.

Nevertheless, when a revision of the book in which my paper had first appeared was proposed two decades later I decided, with some relief, to drop this chapter altogether and replace it with an altogether different chapter (Danziger, 2001a). What were my reasons? Quite soon after the appearance of the first chapter I came to recognize the severe limitations of a historical approach based on the identification of "intellectual traditions". Logically, such an approach has much in common with psychological explanations of human behavior in terms of instincts, needs, or drives identified by such labels as "aggressive", "acquisitive", "submissive", "gregarious", and so on. These are terms used to *classify* behavior into certain categories, but their reification in the form of hypothetical entities provides at best a spurious form of *explanation*. Similarly, though it may be useful for descriptive purposes to note the way in which certain concepts resemble one another, the reification of those resemblances as an intellectual tradition does not in itself explain anything. Of course, if the hypothetical "tradition" leads to detailed research that establishes the existence of significant historical links between the members of that tradition, the hypothesis will at least have served a useful heuristic function. Unfortunately, in the field of historical psychology, the reification of "traditions", "mindscapes", "mentalities" and the like, has too often proved to be a source of pseudo-explanations than of new historical knowledge.

INVESTIGATIVE PRACTICES

So far, I have identified the critique of justificationist historical narratives as a task for which disciplinary historians may be particularly well placed. But this is a task with limited goals, limited means, and limited relevance. In pursuing this task historians apply themselves to subject matter that has already been historicized. They are presented with a much more challenging and potentially more important task in the form of historicizing the current practices of the discipline. This is the task on which most of the contributions to this volume converge and which has also been the main focus of my own work.

When I first began to work in the field of disciplinary history I accepted uncritically the conventional categories used within the discipline for the self-description

of its activities. My background in the classical European sociology of knowledge (Louw, this volume), had predisposed me to take an interest in the social contextualization of disciplinary activities, but it did not occur to me till later that the very description, the characterization, the bringing into focus of those activities was already problematic, so that their appropriate contextualization would depend on how one had conceived of them in the first place.

Activities that generate psychological knowledge in a disciplinary and institutional context result in certain products that are clearly distinguished from one another by strict social conventions. Some of these products are classified as "theories", others as "observations", others still as "mental tests", or "experiments". Those products are marketable in psychological journals or in other forms and yield credit that is convertible into career advancement and reputation (Bourdieu, 1988). Psychologists, like their colleagues in other fields, use the social distinctions among these *products* as a basis for describing and understanding their *actions* in generating them. So they see themselves as engaged in observing, theorizing, experimenting, test construction, and so on. That is fine for functioning adequately *within* a particular regulatory social framework. But it is not fine if one stops taking this framework for granted, steps outside it, and asks how such an activity-framework complex could come to be. We know it is not an eternal and unalterable necessity of human nature, so we, as disciplinary historians, must inquire into the conditions for its existence. For this purpose the conventional categories of disciplinary self-understanding are inadequate—they are part of the problem, not the solution.

Smooth day to day activity within the taken for granted disciplinary framework relies on a pattern of discourse that makes this framework invisible (Steele & Morawski, 2002), whereas, if the historicity of this framework is to be investigated, it must first be made visible. This requires a different kind of discourse. First of all, the social embeddedness of all such activities as theory construction, experimentation, quantification, and so on, must be recognized. These are all *social practices*, though when defined by their products, they are other things as well. The historicity of the disciplinary framework can be made visible by studying the history of the relevant social practices. I began to do this in the nineteen eighties with studies of the social practices of experimentation, quantification, data analysis, and so forth (Danziger, 1985a, 1987a, 1990a).

In studying the historical trajectory of the psychological experiment as a social institution I was trying to fill a historiographic void that had been created by the way questions of methodology were commonly treated within the discipline. Looking at textbook treatments of methodology one would never guess that a technique like experimentation had a history. Yes, the adoption of experimentation had occurred at a certain point in history, but there was no sense of this "experimentation" being itself a historical entity that changed quite significantly over the years. In part of course this ahistorical view of experimentation resulted from the didactic, generally

unscholarly, treatment of the topic in terms of rules engraved in stone. Harping on the fact that these rules were recent human inventions, replacing other such rules, would only have sown confusion and undermined the faith that methodological instruction was meant to engender.

The traditional presentation of experimentation was not only ahistorical, it was also asocial. There was some recognition of social *psychological* factors as a source of experimental "artifacts", but blindness to the fact that experimental situations were inherently social in character and that their products were social products. Recognizing *that* would have cast grave doubts on the universalistic knowledge claims commonly made on the basis of experimental data gathered under quite specific local conditions.

Another aspect of the prevailing professional ideology which appeared highly problematic was the implication, strongly suggested by methodological teaching and practice, that there was only one kind of knowledge compatible with psychology's prized scientific status. This was the kind of knowledge that was tied to the employment of the techniques of experimental design and statistical analysis which had been widely adopted around the middle of the 20th century. Even a cursory examination of psychology's modern history shows, however, that these beliefs were a later development and were preceded by quite different conceptions regarding scientific psychological knowledge and how it was to be achieved. It therefore seemed appropriate to undertake an analysis of the major variants of psychological experimentalism that had emerged during the foundational period of the discipline's history.

Such an analysis would focus on experimental situations as sites for generating psychological knowledge. Different forms of this knowledge would be produced by different arrangements within experimental situations. As one is dealing with social situations, these arrangements would be social in character, affecting the normed relationship between the participants. These socially structured experimental sites are a sort of workshop specifically designed to come up with a certain product, namely, psychological knowledge of a particular kind. So there are two sides to experiments as social institutions—there is their internal social structure involving a certain distribution of power and tasks, and there is their social function as sites for the making of a product that is accorded some value outside the laboratory.

One set of historical influences that affected changes in experimental practices certainly emanated from psychological work in non-academic settings, so-called "applied" or "practical" work (Danziger, 1987b). In that sense, van Hoorn (this volume) is correct in emphasizing the role played by "fields of psychological practice". But this understanding of "practice" harks back to a time when research in academic institutions was not regarded as an example of social practice. Historical and sociological studies of science (see Golinski, 1998 for an overview) have long abandoned this usage for one which depends on the recognition that the activities

which create science are no less examples of social practice than the activities which create more efficient work environments or less repressed individuals.

My studies of experimental practice arose out of an interest in the microsociology of scientific knowledge, but they also attempted to answer purely historical questions regarding the emergence of certain patterns of psychological investigation in the latter part of the 19th century. This accounts for the labels I adopted to identify various kinds of experimentation in the early days of modern psychology. They were based on the places and the figures that had been historically most closely associated with the emergence of these paradigmatic patterns of investigation, Leipzig and Paris as well as Galton and Wundt. My analysis was also limited to a particular time period that ended in the middle of the 20th century. Extending this period, and switching from historical origins to other criteria, Pieter van Strien has emerged with new labels for various identifiable patterns of experimentation. This strikes me as a sensible development. Any expansion of the scope of studies in this field to include new places and periods, as well as new perspectives, is likely to be reflected in new classifications and a new nomenclature.

Thus, van Strien proposes a fundamental distinction, based on strictly *methodological* criteria, between a "natural science model" and a "differential model" of psychological investigation. However, there is more than a difference of terminology behind the fact that he consistently refers to "methodology" where I would prefer to look at "investigative practice". My historical studies of experimental situations had their origin, at least in part, in my dissatisfaction with the disciplinary category of "methodology" that depended on the isolation of certain formal relationships from the social practices that constituted investigative situations. The category "investigative practice" was meant to encompass the social as well as the methodological aspect of these situations. The criteria for distinguishing between different sorts of investigative situations would then be social and historical as well as "methodological". Although van Strien recognizes the existence of social factors, it seems to me that his chapter presents the history of psychological investigation largely in terms of purely methodological changes.

Van Strien's chapter makes an important contribution in raising the question of historical continuity as it pertains to some of the more recent investigative practices employed in cognitive psychology. He regards the method of "protocol analysis", used to generate data applied in the construction of various AI programs and expert systems, as a continuation, or at least a revival, of the "systematic introspection" practiced by the Würzburg School at the beginning of the 20th century. This is surprising, because it runs counter to the investigators' own account of the matter (Ericsson & Crutcher, 1991). Drawing a sharp distinction between "earlier introspective techniques" and "current verbal report techniques" used in protocol analysis, they draw attention to the fact that J.B. Watson, the founder of behaviourism, and not any Würzburger, "was the first investigator to publish an analysis of a think-aloud protocol" (p. 63). It was behaviorism, they claim,

which was responsible for two innovations that made it possible "for psychology to achieve status as a science" (p. 62).

The first of these innovations was the use of subjects untrained in introspective procedures, the second was "the introduction of methods for collecting observations in which trust was not an issue" (p. 63). The point here was that subjects' responses would be limited to the kind of verbalization whose reference could be checked against non-verbal task performance so that one did not have to take the subject's word for anything—trust was not an issue. Clearly, the practice of systematic introspectionism contravened both these requirements. It depended on sophisticated, not naïve, subjects, and it was based on trust in the validity of subjects' introspective reports on their conscious experience.

Both of these distinguishing features, the training of subjects and the trust placed in their reports, are social features. In other words, they involve differences in investigative practice, not merely differences in formal "methodology". Although there are no features of investigative situations that are not "social" in some sense, there are certain core social features which play a crucial role in shaping the overall pattern. In any investigative situation one will find a complex of features, some relatively superficial, such as the number of subjects, others of constitutive significance, such as the social relationship of investigators to their subjects and to the potential consumers of their research product.

For example, the question of whether there is symmetry (equality, exchange of roles) or asymmetry in the relationship of experimenters and subjects is critically linked to questions about what exactly is being studied in the experiments. In asymmetrically constituted "verbal report" studies the subject's activity is analysed in terms of task requirements and the nature of the subject's cognitive processes is determined inferentially by the investigator. This has the far reaching consequence that *descriptions* of knowledge and information are equated with knowledge and information (see Clancey, 1997). By contrast, in symmetrically constituted classical introspection studies the imposed task and the subject's activity were carefully separated and the subject's report was essentially incorrigible.

Van Strien has pioneered a potentially very fruitful extension of the analysis of investigative practice into the latter part of the 20th century. However, the question of whether the dominant cognitivism of that period entailed significant changes in the investigative practices of psychologists remains open. The next step in answering it will require an extension of the analysis beyond the purely "methodological".

PSYCHOLOGICAL OBJECTS

Several contributors to this volume take up the question of how the history of psychology relates to psychological theory. Two mutually incompatible viewpoints

emerge very clearly. Van Rappard argues for a "history of psychological thinking" that is to be separate from the (social) history of the field which is to be left to professional historians. On this view, theorizing is clearly an affair of pure thought whose products are historically autonomous and whose relevance is therefore permanent: the theories of the past are participants in current debates. In contrast to this wonderfully Platonic view, Stam points out that the activity of theorizing is always socially contextualized so that its creations are essentially historical products. As a result, history and theory are deeply interwoven.

My own sympathy for the latter view arose directly out of my background in the sociology of knowledge. But after I began systematically analyzing psychological research publications, starting with the *Philosophische Studien*, I became increasingly aware of the role played by psychologists' investigative practices in mediating the relationship between the broader social context and their own theoretical constructions. The major change that had occurred when psychology became an experimental discipline working with quantitative data was that theories now had to be justified in the court of "scientific" empirical investigation and not by an appeal to philosophical argument or popular intuition. But, to continue the analogy, the judges in this court were biased from the start, because the choice of "methods", of investigative practices, had been largely determined by pre-existing assumptions that were congruent with the theories that were to be tested by means of these methods (Danziger, 1988). When that congruence did not exist, for example in attempts to test Freudian theories experimentally, it was restored by transforming the original theory into one whose presuppositions matched those of the methodology (Danziger, 1985b).

In day to day disciplinary practice theories are not deployed independently of methodological questions and empirical observations. That happens only in texts devoted to "Theory" with a capital T. Outside this rarefied atmosphere theories only exist as part of a complex of constructive activities that includes the planned evocation of phenomena, the intentional suppression of others, the focusing of attention on specific features, the following of particular rules of procedure and of interpretation. Theories in isolation are a creation of metatheory (Danziger, 1993), they are posited as objects of attention for a particular specialty in the academic division of labor.

Outside this specialty, theories have to be studied as part of a complex of social practices that is of course subject to historical change. That complex includes the so-called "data" that are the product of the application of these practices *in* the real world, not *to* the world. This distinction is important because it is not the case that a solitary investigator, occupying a god-like position outside the world, turns his or her gaze on some phenomenon in this world. What happens in psychological practice is that investigators, who are very much *in* the world, apply a complex apparatus of presuppositions, theoretical models, instruments, labels, interpretations, social influence, coercion, numerical and practical skills, etc., to some other

part of the world with which they are in interaction. That application has certain results; it leaves the world in a slightly different state from its state before the psychologists' intervention. Some of these results take the form of empirical data categorized and described in a specific way; other results would show up in the form of certain social relationships peculiar to the investigative context (expressed in the experimental subject role, for example); yet other results might take the form of purely discursive structures and representations.

These changes are not random. When they occur on a large scale, and over longer periods of time, definite patterns become apparent, and these patterns constitute a large part of the subject matter appropriate for a disciplinary history. What shall we call these things that are the product of psychologists' activity *in* the world, these things that would not have existed in quite this way but for psychology's apparatus of intervention? I believe the most appropriate name for them is "psychological objects". They are the objects at which the actions of psychologists qua psychologists are directed: experimental subjects, mental tests, quantitative empirical data, and so forth. But, because these actions are not passive states but interventions in the world, the objects at which they are directed are also in a significant sense their *products*.

Is this "social constructionism"? Certainly, in the sense that "construction" refers to a generative metaphor (Danziger, 1990b) I have found useful, but certainly not in the sense of a general belief that everything that exists is the product of human construction (Hacking, 1999). The "ism" in constructionism is extremely hard to pin down. At one time I agreed to review a series of about a dozen books on "social construction" because I hoped it would help me to get to the core of this slippery term. I failed because I suspect this category has no core, no prototypical member, only a huge variety of self-proclaimed instances, some of which even lack a family resemblance (Danziger, 1997b). So I doubt that anything is gained by the application of this category label. It seems to me more fruitful to ask what the metaphor of construction can do for the advancement of historical and theoretical studies in psychology.

It is certainly useful in historicizing the conceptual apparatus of the discipline. One of the functions that this apparatus performs is the labeling and identifying of psychological phenomena. These phenomena do not have labels attached to them by nature but are given an identity when psychologists (or lay people) subsume them under one or other psychological category. Those categories are of vital importance in the construction of psychological objects because they define what psychologists consider themselves to be investigating, for example, personality, motivation, or memory. They are also pre-theoretical in that psychological theories are typically theories about objects that have already been given a certain identity by the category label under which they are known. But these category labels carry a great deal of implicit theoretical baggage because they come with rich connotations that they have acquired through their everyday usage. Cultural learning

provides people, including psychologists, with an intuitive understanding of what it means to have a personality, an emotion, a motive, or a memory. The constructions that are commonly thought of as psychological theories are superimposed on this understanding and generally do not place it in doubt. A few theories have done so however, and they may well be the only psychological theories worth having.

Because psychological categories are cultural products they all have a history, and one of the tasks of the disciplinary historians is the explication of that history (Danziger, 1997a, 2001a). In my opinion the mere demonstration that categories of psychological understanding have a history already constitutes a contribution to psychology, and not simply to history, because it suggests lines of psychological research that differ significantly from those based on the common interpretation that these categories are accurate reflections of the deep structure of an ahistorical human nature. Research on 'personality' by means of verbal ratings, for example, would then be recognized as a study of a certain semantic space rather than as an investigation of a bit of reality that has the same sort of objectivity as a chemical compound (cf. Semin, 1990).

But beyond this, historical study of psychological categories can also make more specific contributions to psychological theory. For example, it can help to elucidate the foundational role of root metaphors in theoretical constructions that are meant to account for a set of phenomena identified by a particular category label (Danziger, 2002b).

Many of the categories with which contemporary psychology operates are in fact categories whose modern form is closely tied up with the emergence of psychology as a scientific discipline. The more recent history of categories like "behavior", "learning", or "intelligence" can hardly be separated from the history of modern psychology and its role in society. The relationship between common understandings and scientific usage is a two way street. Psychologists soon ceased to be the passive recipients of folk knowledge and became an increasingly powerful influence on the way people explained their experiences and their actions to themselves (Richards, 2002a; Rose, 1996). In that sense, the twentieth century was indeed the century of psychology. This "looping effect" (Hacking, 1995b) constitutes a further dimension in the construction of psychological objects. Because of the social impact that the past activity of psychologists has had, some of the objects they seek to investigate do not need to be constructed afresh in the laboratory or the therapy room, they walk in ready made, displayed as traits, states and disorders of various kinds. That too provides a useful area of historical investigation (Young, 1995).

Studies along these lines are very much concerned with recent history. I see them as examples of what, in Michel Foucault's felicitous phrase, is now often referred to as "history of the present" which forms the antipole to the historicists' construct of "presentism". Whereas presentism referred to the projection of modern forms of understanding onto people, events, and ideas that flourished long ago,

the history of the present problematizes the taken for granted quality of modern forms of understanding and studies their historicity. This is an undertaking to which psychologist historians can be expected to make a contribution. It will not be quite the same sort of contribution as that of cultural historians, sociologists, or anthropologists who choose to participate in this essentially interdisciplinary undertaking. But it is an undertaking in which psychologist historians have a role.

A HISTORICAL PSYCHOLOGY?

Everything I have said so far about the role of historically oriented psychologists limits their work to that relatively recent period of history in which the discipline of psychology existed or was at least on the historical horizon. However, one of the contributors to this volume (van Hoorn) argues strongly against this limit and another (van Rappard) adopts a historical approach (in terms of intellectual traditions) for which no such limit exists. I believe this is a genuine issue which needs to be addressed.

Within psychology, opposition to the very notion of a historical psychology has taken two very different forms. The first kind of opposition arises out of the fact that in their everyday research and theory testing most psychologists proceed as though their subject matter belonged to a historically constant human nature. In their world there is simply no room for a historical psychology. However, belief in the trans-historical validity of experimentally produced psychological knowledge will remain a matter of faith, not of science, until its reasonableness can be empirically demonstrated. But any such demonstration will require considerable reliance on historical evidence. In other words, the scientific grounding of the belief that historical psychology is redundant would itself require evidence from historical psychology. This does not appear to offer a sound basis for rejecting the field.

A variant of this position is based on simple minded scientism, the notion that the only way to obtain valid knowledge is by means of quantitative and experimental procedures. Historical psychology is largely unable to make use of such procedures and is therefore outside the pale. Virtually the whole of the humanities and parts of the social sciences will of course be rejected on the same grounds. Again, this is essentially a confession of faith that is more likely to produce puzzlement than assent among those outside the faith.

There is however a much more telling argument against the backward extension of the history of psychology into pre-disciplinary times so as to produce a historical psychology. This argument is based on the concern that there are no acceptable criteria which might define the subject matter of such a field (Richards, 1987; Smith, 1988). Those who have these concerns are in no doubt about the deep

historicity of matters psychological, but they are then confronted by the dilemma that, in one sense, almost everything in human history pertains to psychology, yet, in another sense, almost nothing does, except during the last century or two. Psychology's status as a natural science, and hence its commitment to certain methodologies, impose fairly clear limits on its territory. But if we were to drop these restrictions and regard all the historical expressions of human nature as fair game, then historical psychology would certainly become a field without boundaries and without discipline. From a historicist perspective it is also argued that the discipline of psychology is itself a historical formation, a way of regarding the world and a way of acting that is a product of a particular historical context, and a relatively recent one at that. If that is so, then we are not entitled to inflict this modern psychological perspective on times when it did not exist. Doing so would amount to a distortion of the historical facts. But in that case historical psychology lacks legitimacy if it is pursued beyond the most recent period of human history.

I regard these as very telling arguments, but do they spell the death knell of anything like a historical psychology? If one does not wish to flounder in a field that is totally amorphous, nor end up as a crude presentist, are there any lines of work that might constitute an acceptable form of historical psychology? I believe that if one follows a few general guidelines some promising possibilities do open up in this field. In the first place, one must not expect historical psychology to be a unified field: no "grand narratives" purporting to describe the historical evolution of the human mind but rather a collection of studies tracing the historical background of specific psychological objects in particular contexts. The already existing studies of the history of emotions provide some nice examples (Stearns & Stearns, 1988; Harré, 1987). 'Consciousness' might well be a candidate for similar investigations that would begin with its emergence as a discursive object in the 17th century and trace its vicissitudes up to its transformation into an object for experimental intervention in the 19th century.

But restriction to the history of specific objects only constitutes a first step in reconstituting historical psychology as a field of scholarship rather than hazy conjecture. In the past, what proved most damaging to the reputation of the field was the fact that some of its pioneers displayed a rather cavalier attitude to questions of evidence, relying excessively on the intuitive interpretation of arbitrarily selected examples. This was usually linked to a clinical approach to history, more often found among psychiatrists than psychologists, which often claimed to have direct access to the subjectivity of persons living centuries before our time (e.g. van den Berg, 1961). Its disregard for the ordinary precautions of both historical scholarship and clinical practice did not endear the work of these historical psychiatrists to specialists in those areas, though it was popular among general readers. Today, the certainties that this approach promised seem quaint, essentially because they were undermined by developments that fostered a more skeptical, a more critical, approach to the problems of historical psychology.

Among these developments one is of particular importance; it has to do with the role of language and other semiotic signs. Claims of direct intuitive access to the subjective experience of people who lived in bygone times appear plausible only as long as one treats language and other semiotic media as completely transparent. All that is left of those times are linguistic traces, symbols and artifacts, not a single living person. That has serious consequences for the kinds of conclusions we can draw. We cannot confirm our understanding of their world by questioning them or by arriving at a mutually agreed interpretation. We are trapped in the semiotic, and particularly the textual, evidence. What we have are their descriptions of their lives and the artifacts they produced.

This leads to the recently much discussed problem of historical rediscription (Hacking, 1995a; Haddock, 2002; Sharrock and Leudar, 2002), because, as we go back in time, our descriptions and labels for actions, experiences and artifacts will differ more and more profoundly from the accounts and representations a person living at that time would have produced. We therefore have to distinguish between *our* accounts, that we might consider to be true *of* past times, and *their* accounts, which might have been true *in* those times. If we substitute the one for the other we end up with those pseudo-insights that have given historical psychology a bad name. Such a substitution is almost bound to happen if we treat discourse as transparent and pretend to have direct access to past subjectivities, mentalities, etc.

A major part of historical psychology's empirical basis comes in the form of texts, more often than not published texts. But, whether public or private, the contents of texts are circumscribed by historically variable norms and conventions of discursive form. As long as we restrict ourselves to the discursive practices involved in the production of the textual evidence we are on relatively firm ground. But I am afraid that any historical psychology which avoids this ground in favor of claims about the feelings or the private experience of an age will not be taken seriously. What texts do offer the historical psychologist are discursive objects embedded in a discursive domain. The history of these discursive realities is a history of objects, not a history of subjects.

The same applies to the non-linguistic evidence, symbolic and artifactual, that a historical psychology would have to work with. There are social and material practices that cannot simply be assimilated to the category of discourse. Therefore, in my most recent work on the history of "memory" as a psychological object, I have considered it necessary to examine the role of the social practices known as "mnemonics" and material practices used in the apparatus of external memory (Danziger, 2002b) as well as strictly discursive practices, such as the deployment of metaphor (Danziger, 2002a).

Because of the Cartesian tradition of treating psychological objects as either natural objects or as subjectivities, their history as discursive objects has been neglected. That leaves historians with a twofold critical task. On the one hand, they need to investigate the role of material and discursive practices in conferring

whatever historical persistence psychological objects possess. On the other hand, they need to question the tendency to credit psychological objects with much greater historical persistence than they in fact possess and to make visible the extraordinary historical mutability of these objects.

LOOKING AHEAD

Charting the vicissitudes of investigative situations in terms of their social structure and function certainly plugs a big hole in the traditional historiography of psychology. However, this field of study is not simply of historical interest. As Richard Walsh-Bowers shows, the social contextual approach to the conduct of psychological research has important implications for current issues and future trends. He devotes particular attention to the topic of research ethics, suggesting that "it exists to legitimize conventional methodological practices" (p. 104). Not content with merely noting this state of affairs, he concludes his chapter with a thought provoking section on "the potential for change", meaning change in the direction of a less dehumanized form of research based on more participatory research relationships. I am entirely in sympathy with his activist orientation, but I believe his analysis of the levers of change needs amplification in one important respect.

The question of changing a dehumanizing investigative practice so as to accord greater respect to those on whom it was imposed is not a new one for modern psychology. In fact, this question assumed considerable political significance three or four decades ago. I am referring to the anti-test movement, which not only resulted in serious legal challenges to psychological testing but also led to changes in this practice which at least limited the potential for future abuse (Rogers, 1995). Undeniably, there are important differences in the social impact of psychological testing and psychological research which make it problematic to draw an analogy between the two. But there is one aspect of the earlier conflict which I believe does have some relevance for the issue Walsh-Bowers is concerned about. From the history of the anti-test movement and its consequences we learn that, in the main, the role of the psychologists was essentially reactive. Although some of them may have had some criticisms of testing practice the effective impulse for change did not come from them but from those who had been wronged by that practice. Only when there was a significant withdrawal of support from those on whose participation the practice of testing depended were conditions created in which changes advocated by internal critics were actually implemented. I suspect that unless a similar situation develops with respect to research practice there will be severe limits to the changes one can realistically expect.

As Walsh-Bowers notes, the maintenance of prevailing orthodoxy with respect to research practice depends on "deeply internalized modernist norms (are)

powerfully present among psychologists concerning what constitutes 'rigorous' research" (p. 116). But beliefs about the demands of science and scientific research are not limited to psychologists, they exist also among those who participate in research situations as "subjects". If a dramatic illustration of the power of such beliefs is needed, the notorious Milgram experiments will supply it. In that case those who were the targets of experimental manipulation demonstrated a faith in the scientific credentials of psychological research that was strong enough to be compatible with the apparent infliction of severe pain. As long as "investigators" and "subjects" share a similar set of beliefs about the value of "rigorous" research and what it entails the situation is likely to remain relatively stable. Of course, this does not exclude cosmetic changes in the style of research reporting, as Walsh-Bowers notes for community psychology. It is true that a participatory style of research lends itself to what he calls "relational reporting", but the converse does not hold. Merely changing the rules of reporting will not necessarily produce any essential change in the relationships among participants in an investigative situation.

For change to occur, widely shared beliefs would have to become problematic. This might happen in a number of ways. Theoretically, there could be a revolutionary change in the ambient culture, such that humane conduct comes to be valued more highly than precision, efficiency, technological advance, and similar idols of the present age. More likely, changes of that kind could affect certain sectors and certain localities. The conditions favouring that kind of development would be quite diverse and difficult to predict. Rather than engage in speculation along such lines I want to indicate another possibility, one which arises out of the link between research ethics and research product.

A recognition of the fundamentally social nature of psychological research situations has been the cornerstone of my work in this area. By contrast, standard practice is based on an understanding of such situations in terms of "methodology", a purely formal description of conditions, manipulations, and outcomes that brackets out all specifically social features. Those features are either relegated to the status of a separate subset of "conditions" or, for many investigators, rendered completely invisible. As Walsh-Bowers mentions, the very notion of a research relationship seemed to baffle many of the investigators he interviewed. This marginalization of the social in investigators' understanding of research situations results, as a matter of course, in the marginalization of research ethics. Ethical considerations arise in social situations. If situations are not experienced as inherently social, ethical questions will either not arise at all (as was the case for many years in psychological research) or they will be relegated to a separate compartment. Only external links will be recognized between this compartment and another quite separate compartment representing "methodology" or "procedure". What cannot be recognized within this framework is the intrinsic connection between research ethics and research procedure because that would imply a recognition

of the inherently social nature of research situations and the artificiality of an autonomous domain of formal "methodology".

Research situations are set up for the sake of some sort of knowledge product. But not all knowledge products are of the same kind. What standard psychological research situations are designed to produce is what is called "scientific" or "objective" knowledge (often mislabeled "empirical"). The fact that situations have to be specially "designed" to produce that kind of knowledge indicates that there is an intimate connection between the nature of the situation and the nature of its knowledge product. Different situations produce different knowledge products, and if it is quantitative, causal, third person psychological knowledge we want we know what sort of investigative situation to set up. One reason why exploitative or coercive research relationships have been tolerated in the past is that their products were so valued that the ethically problematic nature of their origins could be overlooked. But if and when those products come to seem less valuable ethical scruples are less likely to be discounted.

Human situations in which ethical considerations are ignored, dismissed or circumvented are to some degree dehumanizing. Situations in which people get together to create some knowledge product are no exception. In many scientific research situations this may be of little consequence for the nature of the knowledge product. The properties of a chemical will not be altered by the fact that their discovery was based on the unrecognized contribution of underpaid and overworked laboratory assistants. But in psychological research situations with human subjects things are different. In this case, the information on which the knowledge product depends pertains directly to those participants whose role is that of data source. These participants contribute more than their labour to the knowledge generating process—they contribute themselves. If the situation in which this occurs is to some degree dehumanizing that is likely to be reflected in the information they contribute for the construction of the knowledge product. For example, if the investigative situation prevents human data sources from demonstrating any personal autonomy in their actions no information regarding this aspect of human action will find its way into the research product. The knowledge that results will be knowledge pertaining to individuals unable to demonstrate significant aspects of their human potential. Dehumanizing aspects of the research situation lead to dehumanized psychological knowledge. As long as there is a demand for such knowledge these aspects will be tolerated.

What I am suggesting is that the question of change in research ethics cannot be separated from changes in research practice and changes in the kind of knowledge product that is in demand. Questions of research ethics are closely linked to questions of methodology and ultimately to questions of psychology's scientific project. Traditionalists often react to moves for significant change in research ethics as though there was something subversive about them. They are quite right. Potentially, such moves call into question much more than narrowly conceived

ethical issues. What is called into question is the close relationship between the scientific investigation of human persons and positions of power. As a rule, it is the less powerful who are investigated by the more powerful, and to make effective use of the results of such investigations one must again have power.

The best way to defang ethics is to segregate it into a separate compartment, quite distinct from the issues that really matter to traditionalists. That way one can pass off changes in reporting style for changes in what actually happens on the ground.

That might prompt one to ask what hope there is for any significant disciplinary transformation, a question that can only be considered against a wider background. Ultimately, its resolution will depend on social conflicts and developments beyond the confines of the discipline, and these are likely to show considerable local variation. Perhaps the best conditions for a crucial change in investigative practices exist in those quasi-colonial contexts in which the link between social power and research methodology has become glaringly obvious (Smith, 1999).

Whatever impact disciplinary history has, it will not be the same everywhere. The idea that the discipline as a whole might one day be transformed by the efforts of critical disciplinary historians seems to me preposterous. But these historians may be able to play a role in certain local developments, where "local" refers both to geographical and intellectual space. A juxtaposition of the rather pessimistic tone of Bayer's chapter and Richards' (2002b) highly optimistic assessment of the history of psychology as "the discipline of the future" provides a telling illustration of these local differences, American and British in this case.

But, as both Bayer and Staeuble indicate, whatever their outlook, the historians among psychologists will have to pay more attention to issues of disciplinarity than has been the case hitherto. One of the more important tasks facing disciplinary historians is the historicizing of prevailing structures of disciplinary authority and the policing of disciplinary norms and boundaries. Winston's chapter provides an exemplary product of such work. Any serious discussion of disciplinary change must start from the recognition that disciplinarity itself is a relatively recent, eminently historical, phenomenon that has not followed and will not follow the same course everywhere.

In psychology there has been a close link between rigid forms of disciplinarity and an ahistorical approach to the subject matter. But this may be changing. Interdisciplinary structures are multiplying. Within some of these structures historical approaches are not unwelcome; in others they are more unwelcome than ever. The situation has become more fluid, and that is beginning to corrode some of the structures of disciplinary authority and what Staeuble refers to as the "disciplinary construction of reality". This, as she points out, is particularly clear in a post-colonial context.

However, unresolved problems abound. For example, disciplinarity draws much of its strength from self-reproducing social structures, a property not often shared by multidisciplinary structures (Abbott, 2001). What historical knowledge

might be relevant to the possibility of change in this state of affairs? Moreover, the simple replacement of universalist with non-universalist forms of knowledge would replace one set of problems with another, parochialism and non-portability of knowledge being only the most obvious examples. Renewed historical study of the disciplinization of psychology is likely to acquire a new relevance in the context of these problems.

Some years ago I wrote a paper with the title "Does the history of psychology have a future?" If I were to write a follow-up to that paper today I might entitle it "Does disciplinarity have a future?"

REFERENCES

Abbott, A. (2001). *Chaos of disciplines.* Chicago: University of Chicago Press.

Agassi, J. (1963). Towards an historiography of science. *History and Theory*, Beiheft 2, The Hague: Mouton.

Allport, G. W. (1955). *Becoming*. New Haven: Yale University Press.

Ash, M. G. (1983). The self-presentation of a discipline: History of psychology in the United States between pedagogy and scholarship. In L. Graham, P. Weingart & W. Lepenies (Eds.). *Functions and uses of disciplinary histories* (pp. 143–189). Dordrecht: Reidel.

Barbu, Z. (1960). *Problems of historical psychology.* New York: Grove Press.

Blumenthal, A. (1975). A reappraisal of Wilhelm Wundt. *American Psychologist, 30*, 1081–1088.

Blumenthal, A. (1977). Wilhelm Wundt and early American psychology: A clash of two cultures. *Annals of the New York Academy of Sciences, 291*, 13–20.

Boring, E. G. (1929). *A history of experimental psychology.* NewYork: Century.

Bourdieu, P. (1988). *Homo Academicus*. Stanford: Stanford University Press.

Clancey, W. J. (1997). *Situated cognition: On human knowledge and computer representations*. New York: Cambridge University Press.

Danziger, K. (1980). Wundt and the two traditions in psychology. In R. W. Rieber (Ed.), *Wilhelm Wundt and the making of a scientific psychology* (pp. 73–87). New York: Plenum.

Danziger, K. (1985a). The origins of the psychological experiment as a social institution. *American Psychologist, 40*, 133–140.

Danziger, K. (1985b). The methodological imperative in psychology. *Philosophy of the Social Sciences, 15*, 1–13.

Danziger, K. (1987a). Statistical method and the historical development of research practice in American psychology. In: Krüger, L., Gigerenzer, G. & Morgan, M. (Eds.), *The probabilistic revolution II: Ideas in the sciences* (pp. 35–47). Cambridge, Mass.: MIT Press.

Danziger, K. (1987b). Social context and investigative practice in early twentieth century psychology. In M. G. Ash & W. R. Woodward (Eds.), *Psychology in twentieth century thought and society* (pp. 13–33). New York: Cambridge University Press.

Danziger, K. (1988). On theory and method in psychology. In W. J. Baker, L. P. Mos., H. v. Rappard, & H. J. Stam (Eds.), *Recent trends in theoretical psychology* (pp. 87–94). New York: Springer-Verlag.

Danziger, K. (1990a). *Constructing the subject: Historical origins of psychological research.* Cambridge/New York: Cambridge University Press.

Danziger, K. (1990b). Generative metaphor and the history of psychological discourse. In D. E. Leary (Ed.), *Metaphors in the history of psychology* (pp. 331–356). New York: Cambridge University Press.

Danziger, K. (1993). Psychological objects, practice, and history. *Annals of Theoretical Psychology, 8*, 15–47.

Danziger, K. (1994). Does the history of psychology have a future? *Theory and Psychology, 4*, 467–484.

Danziger, K. (1997a). *Naming the mind: How psychology found its language.* London: Sage.

Danziger, K. (1997b). The varieties of social construction: A review. *Theory & Psychology, 7*, 399–416.

Danziger, K. (2001a). Wundt and the temptations of psychology. In R. W. Rieber & D. K. Robinson (Eds.), *Wilhelm Wundt in history: The making of a scientific psychology* (pp. 69–94). New York: Kluwer Academic/ Plenum.

Danziger, K. (2001b). Sealing off the discipline: Wundt and the psychology of memory. In C. D. Green, M. Shore, & T. Teo (Eds.), *Psychological thought in the nineteenth century: The transition from philosophy to science and the challenges of uncertainty* (pp. 45–62). Washington, D.C.: American Psychological Association.

Danziger, K. (2002a). How old is psychology, particularly concepts of memory? *History and Philosophy of Psychology, 4*, 1–12.

Danziger, K. (2002b). *The historical psychology of memory*. The Wallace A. Russell Memorial Lecture, 110th Annual Meeting of the American Psychological Association, Chicago.

Dehue, T. (1995). *Changing the rules: Psychology in the Netherlands, 1900–1985.* New York: Cambridge University Press.

Ericsson, K. A., & Crutcher, R. J. (1991). Introspection and verbal reports on cognitive processes—two approaches to the study of thinking: Response to Howe. *New Ideas in Psychology, 9*, 57–71.

Gergen, K. & Gergen, M. (1984). *Historical social psychology*. Hillsdale, N.J.: Erlbaum.

Golinski, J. (1998). *Making natural knowledge: Constructivism and the history of science*. New York: Cambridge University Press.

Hacking, I. (1995a). *Rewriting the soul: Multiple personality and the sciences of memory*. Princeton, N.J.: Princeton University Press.

Hacking, I. (1995b). The looping effects of human kinds. In D. Sperber, D. Premack & A. J. Premack (Eds.), *Causal cognition: A multi-disciplinary approach* (pp. 351–383). Oxford: Clarendon Press.

Hacking, I. (1999). *The social construction of what?* Cambridge, Mass.: Harvard University Press.

Haddock, A. (2002). Rewriting the past: Retrospective description and its consequences. *Philosophy of the Human Sciences, 32*, 3–24.

Harré, R. (1987). *The social construction of emotions*. Oxford: Blackwell.

Jaeger, S. & Staeuble, I. (1978). *Die gesellschaftliche Genese der Psychologie*. Frankfurt: Campus.

Jüttemann, G. (Ed.) (1986). *Die Geschichtlichkeit des Seelischen*. Weinheim: Beltz.

Klemm, G. O. (1914). *A history of psychology.* New York: Scribner.

Peeters, H. F. M. (1996). *Psychology: The historical dimension*. Tilburg: Syntax.

Richards, G. (1987). Of what is history of psychology a history? *British Journal for the History of Science, 20*, 201–211.

Richards, G. (2002a). The psychology of psychology: A historically grounded sketch. *Theory & Psychology, 12*, 7–36.

Richards, G. (2002b). History of psychology: the discipline of the future. *History & Philosophy of Psychology, 4*, 13–22.

Rogers, T. B. (1995). *The psychological testing enterprise: An introduction*. Belmont: Brooks/Cole.

Rose, N. (1996). *Inventing our selves: Psychology, power, and personhood.* Cambridge: Cambridge University Press.

Samelson, F. (1974). History, origin myth and ideology: 'Discovery' of social psychology. *Journal for the Theory of Social Behavior*, 13, 217–231.

Scribner, S. (1985). Vygotsky's uses of history. In J. V. Wertsch (Ed.), *Culture, communication and cognition: Vygotskian perspectives* (pp. 119–145). New York: Cambridge University Press.

Semin, G. R. (1990). Everyday assumptions, language and personality. In G. R. Semin & K. J. Gergen (Ed.), *Everyday understanding: Social and scientific implications* (pp. 151–175). London: Sage.

Sharrock, W. & Leudar, I. (2002). Indeterminacy in the past? *History of the Human Sciences, 15*, 95–116.

Smith, L. T. (1999). *Decolonizing methodologies: Research and indigenous peoples*. New York: Zed Books.

Smith, R. (1988). Does the history of psychology have a subject? *History of the Human Sciences, 1*, 147–177.

Stearns, C. Z. & Stearns, P. W. (1988). *Emotion and social change*. New York: Holmes & Meier.

Steele, R. S., & Morawski, J. G. (2002). Implicit cognition and the social unconscious. *Theory & Psychology, 12*, 37–54.

Van den Berg, J. H. (1961). *The changing nature of man*. New York: Norton.

Van Strien, P. (1988). De Nederlandse psychologie in het internationale krachtenveld. *De Psycholoog, 22*, 575–585.

Watson, R. I. (1971). Prescription as operative in the history of psychology. *Journal of the History of the Behavioral Sciences, 2*, 311–322.

Wittgenstein, L. (1968). *Philosophical Investigations*, transl. G. E. M. Anscombe. Oxford: Blackwell.

Young, A. (1995). *The harmony of illusions: Inventing post-traumatic stress disorder*. Princeton, NJ: Princeton University Press.

Young, R. M. (1966). Scholarship and the history of the behavioral sciences. *History of Science, 5*, 1–51.

KURT DANZIGER'S PUBLICATIONS

BOOKS

(1997). *Naming the Mind: How Psychology Found its Language.* London: Sage Publications.

(1990). *Constructing the Subject: Historical Origins of Psychological Research.* New York: Cambridge University Press. (Italian translation, 1995).

(1976). *Interpersonal Communication.* New York: Pergamon Press, 1976. (Translations: Spanish, 1982; Italian, 1981).

(1971). *Socialization.* London: Penguin Books. (Translations: Italian, 1972; German, 1974; Danish, 1974: Swedish, 1975).

(1970). *Readings in Child Socialization.* London: Pergamon Press.

CHAPTERS IN BOOKS

(in press). Where theory, history and philosophy meet: The biography of psychological objects. In D. B. Hill & M. J. Kral (Eds.), *About Psychology: Essays at the Crossroads of History, Theory and Philosophy.* New York: SUNY Press.

(2001). Sealing off the discipline: Wundt and the psychology of memory. In C. D. Green, M. Shore, & T. Teo (Eds.), *Psychological Thought in the Nineteenth Century: The Transition from Philosophy to Science and the Challenges of Uncertainty.* Washington, D.C.: American Psychological Association.

(2001). Wundt and the temptations of psychology. In R. W. Rieber & D. Robinson (Eds.), *Wilhelm Wundt in History.* New York: Plenum Press.

(2001). The unknown Wundt: Drive, apperception and volition. In R. W. Rieber & D. Robinson (Eds.), *Wilhelm Wundt in History.* New York: Plenum Press.

(1999). Natural kinds, human kinds, and historicity. In W. Maiers et al. (Eds.), *Challenges to Theoretical Psychology.* Toronto: Captus Press, 1999.

(1997). The historical formation of selves. In R. D. Ashmore & L. Jussim (Eds.), *Self and Identity: Fundamental Issues, The Rutgers Series on Self and Social Identity, vol. I.* Oxford University Press.

(with P. Ballantyne) (1997). Psychological experiments. In W. G. Bringmann, H. M. Lück, R. Miller, & C. E. Early (Eds.), *A Pictorial History of Psychology.* Quintessence Publishing.

(1996). The practice of psychological discourse. In K. J. Gergen & C. F. Graumann (Eds.), *Historical Dimensions of Psychological Discourse.* Cambridge: Cambridge University Press.

(with P. Shermer) (1994). The varieties of replication: A historical introduction. In J. Valsiner, R. van der Veer & M. van Ijzendoorn (Eds.), *Reconstructing the Mind: Replicability in Research on Human Development.* Norwood, N.J.: Ablex.

(1992). Reiz und Reaktion. *Historisches Wörterbuch der Philosophie, vol. 8*, 554–567. Basel: Schwabe.

(1990). Wilhelm Wundt and the emergence of experimental psychology. In G. N. Cantor, J. R. R. Christie, M. J. S. Hodge, & R. C. Olby (Eds.), *Companion to the History of Modern Science*. London: Routledge.

(1990). Generative metaphor and the history of psychological discourse. In D. E. Leary (Ed.), *Metaphors in the History of Psychology*. Cambridge University Press.

(1990). Die Rolle der psychologischen Forschungspraxis in der Geschichte: Eine kontextualistische Perspektive. In A. Schorr & E. G. Wehner (Eds.), *Psychologiegeschichte heute.* Göttingen: Hogrefe.

(1990). The social context of research practice and the history of psychology. In Wm. J. Baker, R. van Hezewijk, M. E. Hyland & S. Terwee (Eds.), *Recent Trends in Theoretical Psychology. Vol. 2*. New York: Springer-Verlag.

(1988). A question of identity: Who participated in psychology experiments? In J. Morawski (Ed.), *The Rise of Experimentation in American Psychology.* New Haven, Conn.: Yale University Press.

(1988). On theory and method in psychology. In W. Baker, L. Rappard & H. Stam (Eds.), *Recent Trends in Theoretical Psychology.* New York: Springer-Verlag.

(1987). Statistical method and the historical development of research practice in American Psychology. In L. Kruger, G. Gigerenzer, & M. Morgan (Eds.), *The Probabilistic Revolution Vol. II: Ideas in the Sciences.* Cambridge, Mass.: MIT Press.

(1987). Apperception. In R. L. Gregory (Ed.), *The Oxford Companion to the Mind.* Oxford University Press.

(1987). Herrmann Ebbinghaus and the psychological experiment. In W. Traxel (Ed.), *Ebbinghaus—Studien 2.* Passau: Passavia.

(1987). Social context and investigative practice in early twentieth century psychology. In M. G. Ash & W. R. Woodward (Eds.), *Psychology in Twentieth Century Thought and Society.* New York: Cambridge University Press.

(1985). Towards a conceptual framework for a critical history of psychology. In H. Carpintero & J. M. Peiro (Eds.), *Psychology in its Historical context: Essays in Honour of J. Brozek.* Valencia: Monografías de la Revista de Historia de la Psicología.

(1985). The problem of imitation and early explanatory models in developmental psychology. In G. Eckardt, W. G. Bringmann, & L. Sprung (Eds.), *Contributions to a History of Developmental Psychology*. The Hague and New York: Mouton.

(1983). Wundt as methodologist. In G. Eckardt & L. Sprung (Eds.), *Advances in Historiography of Psychology*. Berlin: Deutscher Verlag der Wissenschaften.

(1982). Mid-nineteenth century British psycho-physiology: A neglected chapter in the history of Psychology. In M. Ash & R. W. Woodward (Eds.), *Psychology in nineteenth century thought: International cross-disciplinary perspectives.* New York: Praeger.

(1980). Wundt and the two traditions of psychology. In R. W. Rieber (Ed.), *Wilhelm Wundt and the making of a scientific psychology.* New York: Plenum.

(1980). On the threshold of the New Psychology: Situating Wundt and James. In W. G. Bringmann & R. D. Tweney (Eds.), *Wundt Studies/Wundt Studien.* Göttingen: C. J. Hogrefe.

(1980). Wundt's theory of behavior and volition. In R. W. Rieber (Ed.), *Wilhelm Wundt and the Making of a Scientific Psychology*. New York: Plenum.

(1979). The social origins of modern psychology: Positivist sociology and the sociology of knowledge. In A. R. Buss (Ed.), *The social context of psychological theory: Towards a sociology of psychological knowledge*. New York: Irvington.

(1979). Attitudes to parental control and adolescents' aspirations: A comparison of immigrants and non-immigrants. In K. Ishwaran (Ed.), *Childhood and adolescence in Canada*. Toronto: McGraw-Hill Ryerson.

(1975). Differences in acculturation and patterns of socialization among Italian immigrant families. In E. Zureik & M. Pike (Eds.), *Socialization and Values in Canadian Society, vol. I: Political Socialization*. Toronto: McClelland & Stewart.

(1971). Modernization and the legitimation of social power. In H. Adam (Ed.), *South Africa. Sociological perspectives*. London: Oxford University Press.

ARTICLES IN JOURNALS

(2002). How old is psychology, particularly concepts of memory? *History & Philosophy of Psychology, 4*, 1–12.

(2001). Making social psychology experimental: A conceptual history, 1920–1970. *Journal of the History of the Behavioral Sciences, 36*, 329–347.

(with J. Louw) (2000). Psychological practices and ideology: The South African case. *Psychologie & Maatschappij, 50*, 50–61.

(1998). On historical scholarship: A reply to Dehue. *Theory & Psychology, 8*, 669–671.

(1997). The future of psychology is not its past: A reply to Rappard. *Theory & Psychology, 7*, 107–112.

(1997). The varieties of social construction: A review. *Theory & Psychology, 7*, 399–416.

(with K. Dzinas) (1997). How psychology got its variables. *Canadian Psychology, 38*, 43–48.

(1995). Neither science nor history? *Psychological Inquiry, 6*, 115–117.

(1994). Does the history of psychology have a future? *Theory & Psychology, 4*, 467–484.

(1993). The social context of research practice and the priority of history. *Psychologie und Geschichte, 4*, 178–186.

(1993). Psychological objects, practice, and history. *Annals of Theoretical Psychology, 8*, 15–47.

(1993). History, practice and psychological objects. Reply to commentators. *Annals of Theoretical Psychology, 8*, 71–84.

(1992). Ideas and constructions: Reply to commentators. *Theory and Psychology, 2*, 255–256.

(1992). The project of an experimental social psychology: Historical perspectives. *Science in Context, 5*, 309–328.

(1991). Guest editor's introduction. *History of the Human Sciences, 4*, 327–333. (Special issue on historiography of psychology).

(1990). Malthus for Psychology? *Canadian Psychology, 31*, 276–278.

(1989). Psychological experimentation as social practice: Historical considerations. (In Dutch). *Kennis en Methode, 13*, 146–158.

(1987). New paradigm or metaphysics of consensus? *New Ideas In Psychology, 5*, 13–17.

(1985). Origins of the psychological experiment as a social institution. *American Psychologist, 40*, 133–140.

(1985). The methodological imperative in psychology. *Philosophy of the Social Sciences, 15*, 1–13.

(1983). Origins of the schema of stimulated motion: Towards a pre-history of modern psychology. *History of Science, 21*, 183–210.

(1983). Origins and basic principles of Wundt's Völkerpsychologie. *British Journal of Social Psychology, 22*, 303–313.

(1980). Wundt's psychological experiment in the light of his philosophy of science. *Psychological Research, 42*, 109–122.

(1980). The history of introspection reconsidered. *Journal of the History of the Behavioral Sciences, 16*, 240–262.

(1979). The positivist repudiation of Wundt. *Journal of the History of the Behavioral Sciences, 15*, 205–230.

(1977). Images from the past: The dialectics of control. *Ontario Psychologist, 9*, 6–15.

(1977). Hostility management and ego involvement in discussion groups. *Journal of Social Psychology, 102*, 143–148.

(1974). The acculturation of Italian immigrant girls in Canada. *International Journal of Psychology, 9*, 129–137.

(with H. Morsbach) (1967). Personal style in planning. *Journal of General Psychology, 76*, 167–177.

(1965). The effect of variable stimulus intensity on estimation of duration. *Perceptual and Motor Skills, 20*, 505–508.

(1965). Two kinds of variability in a flicker fusion discrimination task. *Journal of General Psychology, 73*, 37–42.

(1963). Validation of a measure of self-rationalization. *Journal of Social Psychology, 59*, 17–28.

(1963). The psychological future of an oppressed group. *Social Forces, 42*, 31–40.

(1963). Ideology and utopia in South Africa: A methodological contribution to the sociology of knowledge. *British Journal of Sociology, 14*, 59–76.

(1963). Some social psychological aspects of economic growth. *South African Journal of Science, 59*, 394–398.

(1963). Reliability of time estimation by the method of reproduction. *Perceptual and Motor Skills, 16*, 879–884.

(1960). Independence training and social class in Java, Indonesia. *Journal of Social Psychology, 51*, 65–74.

(1960). Parental demands and social class in Java, Indonesia. *Journal of Social Psychology, 51*, 75–86.

(1960). Choice of models among Javanese adolescents. *Psychological Reports, 6*, 346.

(1959). Über das Verhältnis des modernen Behaviorismus zur Lehre Pawlows. *Psychiatrische, Neurologische, Medizinische Psychologie, 11*, 150–158.

(1958). Children's earliest conceptions of economic relationships. *Journal of Social Psychology, 47*, 231–240.

(1958). Value differences among South African students. *Journal of Abnormal and Social Psychology, 57*, 339–346.

(1958). Self-interpretations of group differences in values. *Journal of Social Psychology, 47*, 317–323.

(1957). The child's understanding of kinship terms: A study in the development of relational concepts. *Journal of Genetic Psychology, 91*, 213–231.

(with M. Mainland) (1954). The habituation of exploratory behaviour. *Australian Journal of Psychology, 6*, 39–51.

(1953). The interaction of hunger and thirst in the rat. *Quarterly Journal of Experimental Psychology, 5*, 10–21.

(1951). The operation of an acquired drive in satiated rats. *Quarterly Journal of Experimental Psychology, 3*, 110–132.

MISCELLANEOUS PUBLICATIONS

(2001). Introspection, history of the concept. *International Encyclopedia of the Social & Behavioral Sciences*. Oxford: Elsevier.

(1997). The moral basis of historiography. *History and Philosophy of Psychology Bulletin, 9*(1), 6–15.

(1987). *Psychology for whom?* Michael Keenan Memorial Lecture, St. Thomas Moore College, Saskatoon: University of Saskatchewan.

(1977). Sources of instability in the distribution of control among Italian immigrant and non-immigrant families in Canada. York University Department of Psychology Reports, no. 56.

(1971). The Socialization of Immigrant Children. Institute for Behavioural Research, York University.

(1960). *Psichologi dan Masjarakat* (Psychology and Society). Penerbitan Universitas Gadjah Mada, Indonesia. (In Indonesian).

(1956/7). Some attitudes of South African University students towards the future. Proceedings of the South African Psychological Association, no. 7/8.

INDEX